SCIENCE

FRIDAY, AUGUST 25, 1899.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

*A PROBLEM IN AMERICAN ANTHROPOLOGY.**

WHILE engaged in writing the address which I am to read to you this evening the sad news reached me of the death, on July 31st, of our President of five years ago, Doctor D. G. Brinton. Although not unexpected, as his health had been failing since he was with us at the Boston meeting, where he took his always active part in the proceedings of Section H and gave his wise advice in our General Council, yet his death affects me deeply. I was writing on a subject we had often discussed in an earnest but friendly manner. He believed in an all-pervading psychological influence upon man's development, and claimed that American art and culture were autochthonous, and that all resemblances to other parts of the world were the results of corresponding stages in the development of man; while I claimed that there were too many root coincidences, with variant branches, to be fully accounted for without also admitting the contact of peoples. Feeling his influence while writing, I had hoped that he would be present to-night, for I am certain that no one would have more readily joined with me in urging a suspension of judgment, while giving free expression to opinions, until the facts have been worked over anew and more knowledge attained.

* Address of the retiring President of the American Association for the Advancement of Science, given at Columbus on August 21, 1899.

His eloquent tongue is silent and his gifted pen is still, but I urge upon all who hear me to-night to read his two addresses before this Association: one as Vice-President of the Anthropological Section in 1887, published in our 36th volume of *Proceedings;* the other as retiring President in 1895, published in our 44th volume. In these addresses he has, in his usual forceable and comprehensive manner, presented his views of American anthropological research and of the aims of anthropology.

Dr. Brinton was a man of great mental power and erudition. He was an extensive reader in many languages and his retentive memory enabled him to quote readily from the works of others. He was a prolific writer, and an able critic of anthropological publications the world over. Doing little as a field archæologist himself, he kept informed of what was done by others through extensive travels and visits to museums. By his death American anthropology has suffered a serious loss, and a great scholar and earnest worker has been taken from our Association.

In the year 1857 this Association met for the first time beyond the borders of the United States, thus establishing its claim to the name American in the broadest sense. Already a member of a year's standing, it was with feelings of youthful pride that I recorded my name and entered the meeting in the hospitable city of Montreal; and it was on this occasion that my mind was awakened to new interests which in after years led me from the study of animals to that of man.

On Sunday, August 16th, while strolling along the side of Mount Royal, I noticed the point of a bivalve shell protruding from roots of grass. Wondering why such a shell should be there and reaching to pick it up, I noticed, on detaching the grass roots about it, that there were many other whole and broken valves in close proximity —too many, I thought, and too near together to have been brought by birds, and too far away from water to be the remnants of a musk-rat's dinner. Scratching away the grass and poking among the shells, I found a few bones of birds and fishes and small fragments of Indian pottery. Then it dawned upon me that here had been an Indian home in ancient times and that these odds and ends were the refuse of the people—my first shell-heap or kitchen-midden, as I was to learn later. At the time, this was to me simply the evidence of Indian occupation of the place in former times, as convincing as was the palisaded town of old Hochelaga to Cartier when he stood upon this same mountain side more than three centuries before.

At that meeting of the Association several papers were read, which, had there been a Section of Anthropology, would have led to discussions similar to those that have occurred during our recent meetings. Forty-two years later we are still disputing the evidence, furnished by craniology, by social institutions and by language, in relation to the unity or diversity of the existing American tribes and their predecessors on this continent.

Those were the days when the theory of the unity of all American peoples, except the Eskimo, as set forth by Morton in his 'Crania Americana' (1839) was discussed by naturalists. The volumes by Nott and Gliddon, 'Types of Mankind' (1854) and 'Indigenous Races of the Earth' (1857), which contains Meigs' learned and instructive dissertation, 'The Cranial Characteristics of the Races of Men,' were the works that stirred equally the minds of naturalists and of theologians regarding the unity or diversity of man—a question that could not then be discussed with the equanimity with which it is now approached. The storm caused by Darwin's 'Origin of Species' had

not yet come to wash away old prejudices and clear the air for the calm discussion of theories and facts now permitted to all earnest investigators. Well do I remember when, during those stormy years, a most worthy Bishop made a fervent appeal to his people to refrain from attending a meeting of the Association, then being held in his city, on account of what he claimed to be the atheistic teachings of science. Yet ten years later this same venerable Bishop stood before us, in that very city, and invoked God's blessing upon the noble work of the searchers for truth.

At the meeting of 1857 one of our early Presidents, the honored Dana, read his paper entitled 'Thoughts on Species,' in which he described a species as "a specific amount or condition of concentrated force defined in the act or law of creation," and, applying this principle, determined the unity of man in the following words:

"We have, therefore, reason to believe, from man's fertile intermixture, that he is one in species, and that all organic species are divine appointments which cannot be obliterated unless by annihilating the individuals representing the species."

Another paper was by Daniel Wilson, recently from Scotland, where six years before he had coined that most useful word, 'prehistoric,' using the term in the title of his volume, 'Prehistoric Annals of Scotland.' In his paper Professor (afterward Sir Daniel) Wilson controverted the statement of Morton that there was a single form of skull for all American peoples, north and south, always excepting the Eskimo. After referring to the views of Agassiz as set forth in the volumes of Nott and Gliddon, he said: "Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race, and to prove that, notwithstanding some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar—*a race distinct from all others*. The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents is the supposed uniformity of physiological and especially of physiognomical and cranial characteristics—an ethnical postulate which has not yet been called in question."

After a detailed discussion of a number of Indian crania from Canada and a comparison with those from other parts of America, as described by Morton, Wilson makes the following statements: "But making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of the American race is formed, a little less extravagant to affirm of Europe than of America that the crania everywhere and at all periods have conformed, or even approximated, to one type.

"As an hypothesis, based on evidence accumulated in the Crania Americana, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the northern continent, the conclusions still go far beyond anything established by absolute proof; and the subsequent labors of Morton him-

self, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis."

At the close of this instructive paper are the following words: "If these conclusions, deduced from an examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed: that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi; and in so far as the northern differ from the southern tribes they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux: that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American."

The third paper of the meeting to which I shall refer was by another of our former Presidents, the then well-known student of Indian institutions and the author of the 'League of the Iroquois' (1851). In this paper on 'The Laws of Descent of the Iroquois,' Morgan discusses the League as made up of five nations each of which was subdivided into tribes, and he explains the law of marriage among the tribes, the family relationship and the descent in the female line as essential to the maintenance of the whole system. He then says:

"Now the institutions of all the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still retained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their similarity, kindred ones among the Iroquois and other Indian races of the present day. Their intellectual characteristics, and the predominant features of their social condition, are such as to leave no doubt upon this question; and we believe the results of modern research upon this point concur with this conclusion. Differences existed, it is true, but they were not radical. The Aztec civilization simply exhibited a more advanced development of those primary ideas of civil and social life which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions."

After calling attention to the fact that a similar condition exists among certain peoples of the Pacific Islands, he writes: "Whether this code of descent came out of Asia or originated upon this continent is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence or upon probable induction. Its existence among American races whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction; and would seem to point to Asia as the birthplace of the system."

It would be interesting to follow the succeeding meetings of the Association and note the recurring presentation of views which the quotations I have given show to have been most seriously discussed over a generation ago. An historical review of the literature of American anthropology during the present century would also be interesting in this connection. It is probable, however, that a review of this litera-

ture for the first half of the century would reveal the fact that the writers, with here and there a notable exception, were inclined to theorize upon insufficient data and devoted little time to the accumulation of trustworthy facts. The presentation and discussion of carefully observed facts can almost be said to have begun with the second half of the century, and this is the only part of the subject that now commands serious attention.

A reference to the very latest *résumé* of this subject as presented in the 'History of the New World called America,' by Edward J. Payne, Vol. II., Oxford, 1899, is instructive here. In this volume Mr. Payne expresses his belief in the antiquity and unity of the American tribes, which he considers came from Asia in preglacial and glacial times, when the northwestern corner of America was connected with Asia, and when man "as yet was distinguished from the inferior animals only by some painful and strenuous form of articulate speech and the possession of rude stone weapons and implements, and a knowledge of the art of fire-kindling. Such, it may be supposed, were the conditions under which man inhabited both the Old and the New World in the paleo-ethnic age * * * * Even when a geological change had separated them [the continents] some intercourse by sea was perhaps maintained—an intercourse which became less and less, until the American branch of humanity became practically an isolated race, as America itself has become an isolated continent." (Preface.)

Mr. Payne discusses the growth of the languages of America, the various social institutions and arts, and the migrations of these early savages over the continent, north and south, during the many centuries following, as one group after another grew in culture. He considers all culture of the people autochthonous. "It may, however, be suggested that, as in the Old World, the earlier and the smaller tribes tend to dolichocephaly, while the better developed ones are rather brachycephalous—a conclusion indicating that the varying proportions of the skull should be taken less as original evidence of race than as evidence of physical improvement."

This volume by Mr. Payne is replete with similar statements of facts and theories, and shows how difficult it is for us to understand the complications of the subject before us. It cannot be denied that, taking into consideration the number of authors who have written on this subject, Mr. Payne is well supported in his theory of the autochthonous origin of all American languages, institutions and arts; but the question arises: Has not the old theory of Morton, the industrious and painstaking pioneer of American craniology, been the underlying cause of this, and have not the facts been misinterpreted? At the time of Morton the accepted belief in the unity and universal brotherhood of man was about to be assailed, and it seems, as we now look back upon those times of exciting and passionate discussions, that Morton may have been influenced by the new theory which was so soon to become prominent—that there were several distinct creations of species of the genus *Homo* and that each continent or great area had its own distinct fauna and flora. Certainly Morton ventured to make a specific statement from a collection of crania which would now be regarded as too limited to furnish true results.

The anthropologist of to-day would hardly venture to do more than to make the most general statements of the characters of any race or people from the examination of a single skull; although, after the study of a large number of skulls from a single tribe or special locality, he would probably be able to select one that was distinctly char-

acteristic of the special tribe or group to which it pertained.

Relatively long and narrow heads and short and broad heads occur almost everywhere in greater or less proportion. In determining the physical characters of a people, so far as this can be done from a study of crania, the index of the height of the skull is quite as important as that of its breadth. These indices simply give us the ready means of expressing by figures the relative height and breadth of one skull in comparison with another, a small part of what the zoologist would consider in describing, for instance, the skulls of the different species of the genus *Canis*. So in our craniological studies we should determine the relative position, shape and proportions of the different elements of the skull. In fact, we should approach the study of human crania with the methods of the zoologist, and should use tables of figures only so far as such tables give us the means of making exact comparisons. Here, again, are the anthropologists at a disadvantage, inasmuch as it is only very recently that we are approaching a standard of uniformity in these expressions. It is now more than ever essential that the anthropologists should agree upon a method of expressing certain observed facts in somatology, so that the conscientious labors of an investigator who has had a special opportunity for working upon one group of man may be made available for comparison by investigators of other groups.

Probably the old method, still largely in vogue, of stating averages is responsible for many wrong deductions. If we take one hundred or more skulls of any people we shall find that the two extremes of the series differ, to a considerable extent, from those which naturally fall into the center of the series. These extremes in the hands of a zoologist would be considered the subvarieties of the central group or variety. So in anthropology we should take the central group of the series as furnishing the true characters of the particular variety or group of man under consideration, and should regard the extremes as those which have been modified by various causes. It may be said that this central group is defined by stating the mean of all the characters, but this is hardly the case, for by giving the mean of all we include such extraneous characters as may have been derived by admixture or from abnormal conditions.

The many differing characteristics exhibited in a large collection of crania, brought together from various portions of America, North and South, it seems to me, are reducible to several great groups. These may be generally classed as the Eskimo type the northern and central or so-called Indian type, the northwestern brachycephalic type, the southwestern dolichocephalic type, the Toltecan brachycephalic type and the Antillean type, with probably the ancient Brazilian, the Fuegian and the pre-Inca types of South America. Each of these types is found in its purity in a certain limited region, while in other regions it is more or less modified by admixture. Thus the Toltecan, or ancient Mexican, type (which, united with the Peruvian, was separated as the Toltecan family even by Morton) occurs, more or less modified by admixture in the ancient and modern pueblos and in the ancient earth-works of our central and southern valleys. In Peru, more in modern than in ancient times, there is an admixture of two principal types. At the north of the continent we again find certain traits that possibly indicate a mixture of the Eskimo with the early coast peoples both on the Pacific and on the Atlantic sides of the continent. The north-central Indian type seems to have extended across the continent and to have branched in all directions, while a similar but not so extensive branching, northeast and south,

seems to have been the course of the Toltecan type.

This is not theorizing upon the same facts from which Morton drew the conclusion that all these types were really one and the same. Since Morton's time we have had large collections of crania for study, and the crania have been correlated with other parts of the skeleton and with the arts and institutions of the various peoples.

Although these relations have been differently interpreted by many anthropologists who have treated the subject, yet to me they seem to indicate that the American continent has been peopled at different times and from various sources; that in the great lapse of time since the different immigrants reached the continent there has been in many places an admixture of the several stocks and a modification of the arts and customs of all; while natural environment has had a great influence upon the ethnic development of each group. Furthermore, contact of one group with another has done much to unify certain customs; while 'survivals' have played an active part in the adoption and perpetuation of arts and customs not native to the people by whom they are preserved.

The Inca civilization, a forcible one coming from the north, encroached upon that of the earlier people of the vicinity of Lake Titicaca, whose arts and customs were, to a considerable extent, adopted by the invaders. It is of interest here to note the resemblance of the older Andean art with that of the early Mediterranean, to which it seemingly has a closer resemblance than to any art on the American continent. Can it be that we have here an æsthetic survival among this early people, and could they have come across the Atlantic from that Eurafric region which has been the birth-place of many nations? Or is this simply one of those psychical coincidences, as some writers would have us believe! The customs and beliefs of the Incas point to a northern origin and have so many resemblances to those of the ancient Mexicans as hardly to admit of a doubt that in early times there was a close relation between these two widely separated centers of ancient American culture. But how did that pre-Inca people reach the lake region? Is it not probable that some phase of this ancient culture may have reached the Andes from northern Africa? Let us consider this question in relation to the islands of the Atlantic. The Canary Islands, as well as the West Indies, had long been peopled when first known to history; the Caribs were on the northern coast of South America, as well as on the islands; and in the time of Columbus native trading boats came from Yucatan to Cuba. We thus have evidence of the early navigation of both sides of the Atlantic, and certainly the ocean between could easily have been crossed.

One of the most interesting as well as most puzzling of the many phases of American archæology is the remarkable development of the art of the brachycephalic peoples, extending from northern Mexico northeastward to the Mississippi and Ohio valleys, then disappearing gradually as we approach the Alleghenies and, farther south, the Atlantic coast, also spreading southward from Mexico to Honduras, and changing and vanishing in South America. Unquestionably of very great antiquity, this art, developed in the neolithic period of culture, reached to the age of metals, and had already begun to decline at the time of the Spanish conquest. How this remarkable development came to exist amid its different environments we cannot yet fully understand; but the question arises: Was it of autochthonous origin and due to a particular period in man's development, or was it a previously existing phase

modified by new environment? For the present this question should be held in abeyance. To declare that the resemblance of this art to both Asiatic and Egyptian art is simply a proof of the psychical unity of man is assuming too much and is cutting off all further consideration of the subject.

The active field and museum archæologist who knows and maintains the association of specimens as found, and who arranges them in their geographical sequence, becomes intimately in touch with man's work under different phases of existence. Fully realizing that the natural working of the human mind under similar conditions will to a certain extent give uniform results, he has before him so many instances of the transmission of arts, symbolic expressions, customs, beliefs, myths and languages that he is forced to consider the lines of contact and migration of peoples as well as their psychical resemblances.

It must be admitted that there are important considerations, both physical and mental, that seem to prove a close affinity between the brown type of eastern Asia and the ancient Mexicans. Admitting this affinity, the question arises: Could there have been a migration eastward across the Pacific in neolithic times, or should we look for this brown type as originating in the Eurafric region and passing on to Asia from America? This latter theory cannot be considered as a baseless suggestion when the views of several distinguished anthropologists are given the consideration which is due to them. On the other hand, the theory of an early migration from Asia to America may also be applied to neolithic time.

However this may have been, what interests us more at this time, and in this part of the country, is the so-called 'Mound Builder' of the Ohio Valley. Let us first clear away the mist which has so long prevented an understanding of this subject by discarding the term 'Mound Builder.' Many peoples in America, as well as on other continents, have built mounds over their dead, to mark important sites and great events. It is thus evident that a term so generally applied is of no value as a scientific designation. In North America the term has been applied even to refuse piles: the kitchen-middens or shell-heaps which are so numerous along our coasts and rivers have been classed as the work of the 'Mound Builder.' Many of these shell-heaps are of great antiquity, and we know that they are formed of the refuse gathered on the sites of the early peoples. From the time of these very early deposits to the present such refuse piles have been made, and many of the sites were reoccupied, sometimes even by a different people. These shell-heaps, therefore, cannot be regarded as the work of one people. The same may be said in regard to the mounds of earth and of stone so widely distributed over the country. Many of these are of great antiquity, while others were made within the historic period and even during the first half of the present century. Some mounds cover large collections of human bones; others are monuments over the graves of noted chiefs; others are in the form of effigies of animals and of man; and, in the South, mounds were in use in early historic times as the sites of ceremonial or other important buildings. Thus it will be seen that the earth-mounds, like the shell-mounds, were made by many peoples and at various times.

There are, however, many groups of earthworks which, although usually classed as mounds, are of an entirely different order of structure and must be considered by themselves. To this class belong the great embankments, often in the form of squares, octagons, ovals and circles, and the fortifications and singular structures on hills and plateaus, which are in marked contrast to

the ordinary conical mounds. Such are the Newark, Liberty, Highbank and Marietta groups of earthworks, the Turner group, the Clark or Hopewell group, and many others in Ohio and in the regions generally south and west of these great central settlements; also the Cahokia Mound opposite St. Louis, the Serpent Mound of Adams County, the great embankments known as Fort Ancient which you are to visit within a few days, the truly wonderful work of stone known as Fort Hill in Highland County, and the strange and puzzling walls of stone and cinder near Foster's Station.

So far as these older earthworks have been carefully investigated, they have proved to be of very considerable antiquity. This is shown by the formation of a foot or more of vegetable humus upon their steep sides, by the forest growth upon them which is often of primeval character, and by the probability that many of these works, covering hundreds of acres, were planned and built upon the river terraces before the growth of the virgin forest.

If all mounds of shell, earth or stone, fortifications on hills, or places of religious and ceremonial rites, are classed irrespective of their structure, contents, or time of formation, as the work of one people, and that people is designated 'the American Indian' or the 'American Race,' and considered to be the only people ever inhabiting America, North and South, we are simply repeating what was done by Morton in relation to the crania of America—not giving fair consideration to differences while overestimating resemblances. The effort to affirm that all the various peoples of America are of one race has this very year come up anew in the proposition to provide 'a name which shall be brief and expressive' and at the same time shall fasten upon us the theory of unity—notwithstanding the facts show diversity—of race.

Let us now return to the builders of the older earthworks, and consider the possibility of their having been an offshoot of the ancient Mexicans. Of the crania from the most ancient earthworks we as yet know so little that we can only say that their affinities are with the Toltecan type; but of the character of the art, and particularly the symbolism expressing the religious thought of the people, we can find the meaning only by turning to ancient Mexico. What Northern or Eastern Indian ever made or can understand the meaning of such sculptures or such incised designs as have been found in several of the ancient ceremonial mounds connected with the great earthworks? What Indian tribe has ever made similar carved designs on human and other bones, or such singular figures, cut out of copper and mica, as were found in the Turner and Hopewell groups? Or such symbolic animal forms, elaborately carved in stone, and such perfect terra cotta figures of men and women as were found on the sacrificial altars of the Turner group? What meaning can be given to the Cincinnati Tablet, or to the designs on copper plates and shell discs from some of the Southern and Western burial and ceremonial mounds? I think we shall search in vain for the meaning of these many objects in the North or East, or for much that resembles them in the burial places of those regions. On the other hand, most of these become intelligible when we compare the designs and symbols with those of the ancient Mexican and Central American peoples. The Cincinnati Tablet, which has been under discussion for over half a century can be interpreted and its dual serpent characters understood by comparing it with the great double image known in Mexico as the Goddess of Death and the God of War. The elaborately complicated designs on copper plates, on shell discs, on human bones and on the wing bones of the eagle

can in many instances be interpreted by comparison with Mexican carvings and with Mexican modes of symbolic expression of sacred objects and religious ideas. The symbolic animals carved on bone or in stone and the perfection of the terra cotta figures point to the same source for the origin of the art.

In connection with the art of the builders let us consider the earth structures themselves. The great mound at Cahokia, with its several platforms, is only a reduction of its prototype at Cholula. The fortified hills have their counterparts in Mexico. The serpent effigy is the symbolic serpent of Mexico and Central America. The practice of cremation and the existence of altars for ceremonial sacrifices strongly suggest ancient Mexican rites. We must also recall that we have a connecting link in the ancient pueblos of our own Southwest, and that there is some evidence that in our Southern States, in comparatively recent times, there were a few remnants of this old people. It seems to me, therefore, that we must regard the culture of the builders of the ancient earthworks as one and the same with that of ancient Mexico, although modified by environment.

Our Northern and Eastern tribes came in contact with this people when they pushed their way southward and westward, and many arts and customs were doubtless adopted by the invaders, as shown by customs still lingering among some of our Indian tribes. It is this absorption and admixture of the peoples that has in the course of thousands of years brought all our American peoples into a certain conformity. This does not, however, prove a unity of race.

It is convenient to group the living tribes by their languages. The existence of more than a hundred and fifty different languages in America, however, does not prove a common origin, but rather a diversity of origin as well as a great antiquity of man in America.

That man was on the American continent in quaternary times, and possibly still earlier, seems to me as certain as that he was on the Old World during the same period. The Calaveras skull, that bone of contention, is not the only evidence of his early occupation of the Pacific coast. On the Atlantic side the recent extensive explorations of the glacial and immediately following deposits at Trenton are confirmatory of the occupation of the Delaware Valley during the closing centuries of the glacial period and possibly also of the interglacial time. The discoveries in Ohio, in Florida and in various parts of Central and South America all go to prove man's antiquity in America. Admitting the great antiquity of one or more of the early groups of man on the continent, and that he spread widely over it while in the palæolithic and early neolithic stages of culture, I cannot see any reason for doubting that there were also later accessions during neolithic times and even when social institutions were well advanced. While these culture epochs mark certain phases in the development of a people, they cannot be considered as marking special periods of time. In America we certainly do not find that correlation with the Old World periods which we are so wont to take for granted.

We have now reached the epoch of careful and thorough exploration and of conscientious arrangement of collections in our scientific museums. It is no longer considered sacreligious to exhibit skulls, skeletons and mummies in connection with the works of the same peoples. Museums devoted primarily to the education of the public in the æsthetic arts are clearing their cases of heterogeneous collections of ethnological and archæological objects. Museums of natural history are being arranged to show the history and distribution of animal

and vegetable life and the structure of the earth itself. Anthropological museums should be similarly arranged and, with certain gaps, which every curator hopes to fill, they should show the life and history of man. To this end the conscientious curator will avoid the expression of special theories and will endeavor to present the true status of each tribe or group of man in the past and in the present, so far as the material at his command permits. A strictly geographical arrangement is, therefore, the primary principle which should govern the exhibition of anthropological collections. A special exhibit may be made in order to illustrate certain methods by which man in different regions has attained similar results, either by contact or by natural means. Another exhibit may be for the purpose of showing the distribution of corresponding implements over different geographical areas. These and similar special exhibits are instructive and under proper restrictions should be made, but unless the design of each exhibit is clearly explained, the average visitor to a museum will be confused and misled, for such objects so grouped convey a different impression than when exhibited with their associated objects in proper geographical sequence.

The anthropology of America is now being investigated and the results are being made known through museums and publications as never before.

The thoroughly equipped Jesup North Pacific Expedition, with well-trained anthropologists in charge, was organized for the purpose of obtaining material both ethnological and archæological for a comparative study of the peoples of the northern parts of America and Asia. Although only in the third year of its active field work, it has already furnished most important results and provided a mass of invaluable authentic material.

The Hyde Expedition planned for long-continued research in the archæology and ethnology of the Southwest, a successor in regard to its objects to the important Hemenway Expedition, is annually adding chapters to the story of the peoples of the ancient pueblos.

The results of the extensive explorations by Moore of the mounds of the southern Atlantic coast are being published in a series of important monographs.

The Pepper-Hurst Expedition to the Florida Keys has given information of remarkable interest and importance from a rich archæological field before unknown.

The United States government, through the Bureau of Ethnology of the Smithsonian Institution, has given official and liberal support to archæological and ethnological investigations in America.

The constantly increasing patronage, by wealthy men and women, of archæological research at home, as well as in foreign lands, is most encouraging.

The explorations in Mexico and in Central and South America, the publication in fac-simile of the ancient Mexican and Maya codices, the reproduction by casts of the important American sculptures and hieroglyphic tablets, all have been made possible by earnest students and generous patrons of American research.

The numerous expeditions, explorations and publications of the Smithsonian Institution and of the museums of Washington, Chicago, Philadelphia, New York and Cambridge are providing the student of to-day with a vast amount of authentic material for research in American and comparative anthropology.

The archæological Institute of America, the American Folk-Lore Society and the archæological and anthropological societies and clubs, in active operation in various parts of the country, together with the several journals devoted to different branches

of anthropology, give evidence of widespread interest.

Universities are establishing special courses in anthropology, and teachers and investigators are being trained. Officers of anthropological museums are preparing men to be field workers and museum assistants. The public need no longer be deceived by accounts of giants and other wonderful discoveries. The wares of the mercenary collector are now at a discount since unauthentic material is worthless.

Anthropology is now a well-established science; and with all this wealth of materials and opportunities, there can be no doubt that in time the anthropologists will be able to solve that problem, which for the past half century has been discussed in this Association—the problem of the unity or diversity of prehistoric man in America.

Frederic Ward Putnam.
Harvard University.

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*THE FIELD OF EXPERIMENTAL RESEARCH.**

Physical research by experimental methods is both a broadening and a narrowing field. There are many gaps yet to be filled, data to be accumulated, measurements to be made with great precision, but the limits within which we must work are becoming, at the same time, more and more defined.

The upper ranges of velocities, temperatures and pressures, which manifest themselves in the study of the stellar universe, are forever beyond the range of experiment. But while the astronomer must wait for opportunities to observe, the experimenter can control his conditions and employ his methods and his apparatus at once to the question in hand. Still this work must be done within a certain range or must be limited to conditions more or less easy to recognize. In spite of this fact, however, the progress made during the past century is not likely to cease or abate in the next, and the ever-increasing number of workers bodes well for the future enrichment of our science.

Whatever may be our ideas of fundamental entities, as expressed in various theories; whether, as an example, we regard the ether as like an infinitely mobile fluid, or as an incompressible solid, or as a jelly; or whether we incline to think that being an electromagnetic medium, it may be without mechanical properties, which properties depend in some way upon the electromagnetic nature of the ether, we cannot reach sure ground without the experimental test.

The development in the field of research by experiment is like the opening of a mine, which, as it deepens and widens, continually yields new treasure, but with increased difficulty, except when a rich vein is struck and worked for a time. In general, however, as the work progresses there will be needed closer application and more refined methods. We may, indeed, find our limit of depth in the mine of experiment in inordinate cost, in temperatures too high, or in pressures beyond the limits of our skill to control.

It is but a few months since Professor Dewar, by the evaporation of liquid hydrogen in a vacuum, closely approached, if he has not reached, our lower limit of possible temperature. Investigations of the effects of low temperature upon the properties of bodies must, from the present outlook, be forever limited to about 20° C. above absolute zero, unless a lighter gas than hydrogen be discovered upon the earth, the actual existence of which it is, of course, impossible to conjecture. Before the actual experimental demonstration of this limit the limit itself was known to theory, at least approximately, but the spur of the experi-

* Address of the Vice-President and Chairman of Section B, Physics, before the American Association for the Advancement of Science at the Columbus meeting, August, 1899.

menter is the overcoming of difficulties and the possibility of new discoveries which come as surprises. In the case in question a liquid of extremely low density, only one-fourteenth that of liquid nitrogen, was produced, while still defined by clear and well-marked refracting surfaces.

When we turn to the consideration of the field for research work at high temperatures we are not confronted by the fact of a physical limit existing which may be approached but never reached. We can imagine no limit to possible increase of temperature, such as is the absolute zero a limit of decrease. While we may actually employ in electric furnaces temperatures which, according to Moissan, have a lower limit of 3,500° C., we can realize the possibility of temperatures existing in the stars measured by tens of thousands or hundreds of thousands of degrees of our temperature scale.

The moderate increase of working temperature given by the electric furnace enabled Moissan and others to reap a rich harvest of experimental results, and the natural inference is that much more might be expected from further extensions of the limits. These limits are, however, already set for us by the vaporization of all known substances. Our furnace itself keeps down the temperature by melting and volatilizing. We may indefinitely increase the energy in an electric arc and thus add to the heat evolved, but the addition only goes to vaporize more material. The limit of work then seems to be readily reached in the electric furnace, no materials for lining being available, not subject either to fusion or vaporization, thus using up the energy which would otherwise go to increase the temperature.

A suggestion as to a possible extension of temperature range may be made here. It may be requisite to work with closed receptacles under pressure, and to discharge through them electric currents of so great energy-value as to attain almost instantaneously the highest temperatures, to be maintained for only a very short time. We may imagine a huge condenser charged to a potential of, say, 10,000 volts as discharged through a limited body of gas contained in a small space within a strong steel tube which has a lining of refractory non-conductor. The energy may thus possibly be delivered so suddenly to a very limited body of material as to result in a momentary elevation of temperature passing all present known limits and capable of effecting profound changes in molecular constitution. We need all possible extension of the limits of research in this direction in order to discover some clue to the relations which the chemical elements bear to each other. The limit of possible strength of the containing receptacle, or some unforeseen factor, would probably set the new bounds. The point to be here enforced, however, is that far beyond any increase of working range in temperature, obtained in any way, there must still exist a further range unattainable by our best efforts and possibly forever outside of the field of experimental research. Our knowledge of this higher range can alone be derived from a study of the actions going on in the stars and nebulæ.

As with the temperature range so it is with the pressure range. We may easily work under conditions which involve no pressure, but when we attempt to conduct our inquiries with increase of pressure we soon find a limit to the tenacity of our strongest vessels or to our ability to produce and maintain extreme pressures. We may work, not easily it is true, with pressures up to a few tons to the square inch, but this is as nothing compared to the conditions which we know must exist within the larger celestial bodies, without reference to their condition, solid, liquid or gaseous. Can we ever hope to experimentally repro-

duce the condition of a mass of gas so compressed that in spite of a very high temperature its volume is less than that of the same mass cooled to solidification? Yet this extreme of condition must be the normal state within the bodies of many of the stars.

It has been aptly said that many, and perhaps most, of the important discoveries have been made with comparatively simple and crude apparatus. While this may be true, yet it is probably true also that future advance work is likely to require more and more refined means and greater nicety of construction and adjustment of apparatus. The expense or cost, if not the difficulty of the work, may become so great as to effectually bar further progress in some fields. When instruments require to be adjusted or constructed, to such refined limits as a fraction of a wave-length of light, but few can be found to undertake the work. The interferometer and echelon spectroscope of Michelson involve such minute adjustments that a wave-length of light is relatively thereto a large measure. It is well known that this comparative coarseness of light waves imposes a limit to the powers of optical instruments, as the microscope and telescope, such that no perfection of proportion, construction and correction of the lenses can remove.

In most fields of research, however, progress in the future will depend in an increasing degree upon the possession, by the investigator, of an appreciation of small details and magnitudes, together with a refined skill in manipulation or construction of apparatus. He must be ready to guide the trained mechanic and be able himself to administer those finishing touches which often mark the difference between success and failure. There must be in his mental equipment that clear comprehension of the proper adjustment of means to ends which is of such great value in work in new fields. He must also learn to render available to science the resources of the larger workshops and industrial establishments.

The application of physical principles upon a large scale in such works has frequently, in recent years, resulted in great gains to science itself. The resources of the physical laboratory are often relatively small and meagre compared with those of the factory. Experimental work in certain lines is now frequently carried on upon a scale so great and under such varied conditions as would be almost impossible outside of a large works.

In no field has this been more true than in that of electricity during the past few years. We need only instance the progress in alternating currents and in relation to the magnetic properties of iron. In large scale operations effects which would be missed or remain masked in work undertaken upon a more restricted scale receive emphasis sufficient to cause them to command attention. The obstacle of increasing costliness of equipment, which in some fields might act as a bar to further progress, can only be overcome by more liberal endowments of laboratories engaged in advance work. Even those in the community who can only understand the value of scientific work when it has been put to practical use may find in the history of past progress that many discoveries in pure science which had not, when made, any apparent commercial importance or value have in the end resulted in great practical revolutions.

Could Volta, when he discovered the pile one hundred years ago, have had any idea of its importance in practical work? Or, did Davy or his contemporaries at the time of his experiments with the arc of flame between the charcoal terminals of his large battery have any suspicion that in less than one hundred years the electric arc would grow to such importance that more

than 100,000 arc lamps would become a single year's production in this country alone. Faraday, when he made his researches upon the induction of electric currents from magnetism, could not have had any idea of the enormous practical work in which the principles he dealt with as facts of pure science would find embodiment. When he wound upon the closed iron ring the two coils of wire which enabled him to discover the facts of mutual induction he had begun, without any suspicion of the fact, the experimental work which gave to science and to practice the modern transformer, now built of capacities ranging up to 2,500 H. P. each, and for potentials of 40,000 to 60,000 volts.

These examples, and many others which might be given, should convince even the most arrogantly practical man of the high value of scientific research, not alone as adding to the sum-total of knowledge and for the admirable training it gives, but because it cannot fail to have an ultimate practical effect. Discoveries which at first seem to have no useful nor practical outcome are often the very ones which underlie development of the greatest importance in the arts and industries.

The work of Hertz upon electric waves was to the physicist a grand experimental demonstration, tending to prove the truth of the electromagnetic theory of light, and subsequent progress was profoundly influenced by it, though no practical use followed at once. The physicist to-day may see in the wireless telegraph only an extension of Hertz's original work, for he need not consider the commercial or economic outcome. He may, however, recognize the fact that in the wireless telegraph, as developed by Marconi, practice calls for a broader theoretical view. Certain elements of construction and adjustment of apparatus, at first used and regarded as essential from a theoretical standpoint, have already been laid aside. The radiator, with its large polished brass spheres and special spark gap, has been found of no more effect than the simple pair of small balls ordinarily constituting the terminals for high potential discharges. It has been found that the transmitting and receiving apparatus do not require to be attuned, and that the receiving coherer is not the true recipient of the electric wave or disturbance in the ether.

These later developments are, in fact, departures, more or less wide, from the principles underlying the Hertz demonstration. A vertical wire is charged to a high potential and discharges to earth over a spark gap. During the discharge the wire becomes a radiator of electromagnetic pulses or waves, regardless of the spark radiation. The receiving vertical wire is likewise alone relied upon to absorb the energy. Being in the path of the electromagnetic wave conveyed in the ether from the transmitting wire, it becomes the seat of electromotive forces which break down the coherer. This, in substance, may be considered as a series of small or microscopic spark gaps which can be crossed by the comparatively low potentials developed in the receiving wire. We are thus taught to recognize the fact that the refinements in methods and apparatus needed for a delicate physical demonstration as of the Hertz waves in this instance may often be laid aside in practical application, where the end to be achieved is different. The sudden discharge of the Marconi transmitting wire may possibly give rise to a series of oscillations or high-frequency alternating waves in the wire, but since the first half of the first wave at each discharge will have the greatest amplitude it is doubtful if those which follow in the short train have any decided effect upon the receiver. According to this view the fact of the discharge being oscillatory may, indeed, have no essential relation to the work done, but

may be an unavoidable incident of the very sudden discharge which itself would set up a single pulse in the ether sufficiently intense for the work even if unaccompanied by lower amplitude oscillations following the first discharge pulse.

Before leaving the consideration of this most fruitful field of experimental research opened by Hertz, it may be stated that the one gap in the work yet to be filled is the actual production of electric waves of a wave-length corresponding to those of the spectrum. If this could be done by some direct method, no matter how feeble the effect obtained, the experimental demonstrations of the electric nature of radiant heat and light would be fitly completed. Several years ago it occurred to me that it might be possible to devise a method for accomplishing the end in view, and so close the existing gap. Many years ago an observation on sound echoes showed clearly the production of high-pitch sounds from single pulses, or lower-pitch waves. A bridge over a mile in length was boarded at the sides, and vertical slats regularly and closely placed along its side formed, for a sound wave incident thereon, a series of reflecting edges or narrow vertical surfaces, a kind of coarse grating. It was found that a loud sound or pulse, such as that of a gun-shot, emanating from a point near one end of the bridge and two to three hundred feet in a line from the structure, was followed by an echo which was in reality a high-pitch musical tone. The pitch of this tone corresponded to the spacing of the slats in the bridge considered as a reflecting grating for sound.

Following this principle, it seems possible that a very sudden pulse in the ether or electromagnetic wave, incident at an angle upon a reflecting grating having from 20,000 to 40,000 ruled lines to the inch, if the plane of incidents were at right angles with the rulings, might be thrown into ripples of the wave-length of light and yield a feeble luminosity. If the color then varied with the angle of incidence chosen and with the angle through which the reflection passed to the eye the experiment would be conclusive.

Despite the diligent studies which had been made in the invisible rays of the spectrum, both the ultrared and ultraviolet, a work far from completion as yet, the peculiar invisible radiation of the Crookes tube remained unknown until the work of Lenard and Röntgen brought it to the knowledge of the world. The cathode discharge, studied so effectively by Hittorf and Crookes, and by the latter called 'radiant matter,' was but a part of the whole truth in relation to the radiation in high vacua. It is needless to recount the steps in the discovery of Röntgen rays. We now know that these rays come from the impingement of the 'radiant matter' or cathode rays. We know, also, that the higher the vacuum, and, therefore, the higher the electric potential needed to effect the discharge, the more penetrating or the less easily absorbed is the resulting radiation. Rays have been produced which in part pass through cast iron nearly an inch thick. The iron acting as a filter absorbs all rays of less penetrating power. A question may here be put, which it will be for future experiment to answer: Can we, by increasing the degree of vacuum in a Crookes tube by the employment of enormous potentials for forcing a discharge through the higher vacuum, produce rays of greater and greater penetrating power? What, in fact, may be the limit, or is there any limit, to the diminution of wave-length in the ether, assuming for the moment that this invisible radiation is somewhat of the same nature as light, but of higher pitch, though it may be unlike light in not representing regular wave trains.

Röntgen radiation, while spoken of as

invisible, is in reality easily visible if of great intensity. The parts of the retina which respond and so give the sensation of luminosity are apparently those around the eye and not directly opposite to the iris opening. Those parts of the retina sensitive to the rays are characterized by the preponderance of 'rods,' giving the simple sensation of illumination, apparently white in the case in question. The 'cones,' or those portions of the retinal membrane whose function is believed to be the recognition of color or differences of wavelength, appear not to be excited by the Röntgen radiation, or only very feebly. If this be true it would account for the less intensity of the luminous effect upon those portions of the retina near the optic axis of the eye. All this favors the view that the Röntgen radiation is without sustained pitch or wave trains, and resembles more a sharp noise or crash in sound.

For pressing experimental work in the highest vacua to its limit, as above suggested, we already have means at command for the production of the most complete exhaustions, requiring extremely high potentials to pass an electric discharge. We have, also, in well-known forms of high-frequency apparatus the means for producing electromotive forces limited only by our means for insulation. A recent apparatus devised by me and called a dynamostatic machine gives equal capability of producing high potentials of definite polarity, positive and negative. It should not be long, therefore, before work is undertaken in this suggested direction of pressing this matter of rays of high penetrating power much farther than has been done. The question arises whether any such rays can exist which are not appreciably absorbed in passing through dense substances. They would probably not affect a photographic plate nor a fluorescent screen. If they lost also the property of ionizing a gas and causing electric convection we might not even be able to discover them. That some influence or action in the ether does actually penetrate the dense masses in space is evidenced by gravitation, the mystery of mysteries. We are, however, not justified in going beyond the proved facts which can only be the result of experimental work and close observation. All else is speculation. The energy source of the Becquerel rays is another mystery apparently far from being cleared up, and if it be true, as recently announced, that a substance named radium has in reality nine hundred times the power of emitting these rays than is possessed by uranium and thorium, and that the radiation is able to cause visible fluorescence of barium platinocyanide, the mystery but deepens and makes us again think of the possible existence of obscure rays only absorbed and converted by a few special substances.

The diffusion which takes place when Röntgen rays pass through various media is another phenomenon which needs more attention from investigators. This effect seems to be produced by all substances in a greater or less degree. It, however, appears to be nearly absent in the case of those substances which give out light or fluoresce under the rays, as barium platinocyanide and calcium tungstate. It will be important to determine definitely whether the rays diffused by different substances are lowered in pitch or penetrating power as compared with the rays exciting the diffusion; whether, in other words, the rays from a tube with quite high vacuum excite similar rays by diffusion, or rays more absorbable; and if a lowering takes place whether it occurs in like manner and degree for all diffusing media.

The phenomenon may be akin to fluorescence, as when quinia sulphate converts the invisible ultraviolet rays of the spectrum into lower rays or visible light. This

action may be at its extreme when barium platinocyanide, excited by Röntgen rays, so lowers the pitch as to produce rays within the visible spectrum, for this compound gives very little or no Röntgen-ray diffusion. Are there substances which under Röntgen rays fluoresce with invisible rays of the order of the ultraviolet of the spectrum? If, as is the case with solid paraffine, the irradiated substance gives rise to considerable diffusion it can, as I have noted, produce a secondary diffusion in other masses of the same substance, or of other substances, as indicated by feeble fluorescence of the sensitive barium salt, thoroughly screened from the direct source of rays and from the first or primary diffusion. It is probable that Tertiary diffusion could be found if we possessed a far more powerful or continuous source of the rays for exciting the initial diffusion. The ray emission, even in the most powerfully excited tube, is probably so intermittent that the active period is but a fraction of the total time. It may easily be that the limit of intensity of Röntgen-ray emission has not yet been reached, especially when artificially cooled anti-cathode plates are available.

There is much room for experimental work in this fascinating field. We need for it the means for the production either of a continuous electric discharge at from 60,000 to 100,000 volts or a high-frequency apparatus capable of giving an unbroken wave train; that is, a succession of high period waves of current without breaks or intermissions.

The ordinary high-frequency apparatus for obtaining discharges of high potential from alternating currents gives only a rapid succession of discharges each consisting of a few rapidly dampened oscillations. These discharges occupy but a small fraction of the total time. This is very different from a continuous sustained wave train, with the successive waves of equal amplitude following each other without break. Such sustained waves will, doubtless, be of use in research, especially in vacuum-tube work, and they would, of course, convey much more energy than the usual brokon or interrupted discharge known as a high-frequency discharge.

Some six or seven years ago I endeavored, while working upon the subject of high frequency, to fill the gap. The result was an apparatus which, with its modifications, deserves more study and experiment than I have been able to give to it. A brief description may not be out of place. A large inductance coil with a heavy iron wire bundle for a core, a coil of relatively few turns with no iron core, and a condenser of variable capacity, were connected in series across the mains of a 500-volt electric circuit. The smaller coreless coil and the condenser were arranged to be shunted by an adjustable spark gap with polished ball terminals. By simply closing for a moment the spark gap so as to form a low resistance shunt around the condenser and the small coil, and afterward slowly separating the balls, the local circuit of the condenser, small coreless coil and shunting gap become the seat of sustained oscillations, the frequency of which depends upon the relation of inductance and capacity in the local circuit. The energy supplied is that of a continuous current through the large inductance coil with the heavy core. The action of the apparatus is easily comprehended by a little study. The oscillating current in the local circuit may be made to induce much higher potentials in a secondary circuit inductively related thereto. In this case the turns of the secondary in relation to the primary are, as usual, such as to step-up the potential. In other words the potential developed in the secondary is determined by the transforming ratio.

We thus have a high-frequency apparatus

in which the waves are sustained in an unbroken series, and we employ as the source of energy a continuous current circuit. It shows that we may continuously supply energy to an oscillating system and so keep up the amplitude of electric oscillations, the frequency of which is that due to the capacity and inductance of the part of the circuit in which oscillations are set up.

While, in the forms of high-frequency apparatus alluded to, we may obtain almost any differences of electric potential up to millions of volts, assuming the apparatus large enough for the work, we do not get a sustained separation of positive and negative charges, as in the static machine, or in a less complete degree with the inductive coil. Professor Trowbridge, of Harvard, has, however, made use of large Planté rheostatic machines, the condenser plates of which are charged in parallel from 10,000 small storage cells connected in series. The discharge of the condenser plates is effected after they are connected in series by a suitable connection changing frame moved for the purpose. Very high potential discharges are thus obtained and the polarity is always definite. It is manifest that the size of the apparatus and the perfection of its insulation determine the possible performance. The objection to such an apparatus for experimental research or demonstration is the large number of cells required and the complicated arrrangements of circuits for charging them. I have, however, recently succeeded in removing all necessity for the presence of charging cells, and have produced what may be termed a dynamostatic machine which is worked by power or by current from a lighting circuit, either continuous or alternating, and may replace a static machine. It is, of course, not dependent upon the weather. I trust it may be of sufficient interest to merit the following brief description: A small electric motor has in addition to its commutator a pair of rings connected to its armature winding for obtaining alternating currents. The shaft of the motor drives synchronously a revolving frame bearing connections which, as in the Planté rheostatic machine, connect a series of condenser plates alternately in parallel for charging and in series for discharging at high potential. A small oil-immersed step-up transformer has its primary connected to the brushes bearing upon the two alternating current rings of the motor, and its secondary, giving say 20,000 volts, is periodically connected to the condenser plates while in parallel, by means of the revolving connection frame. The adjustment is such that only the tops of the alternating waves or their maxima are used to charge the condenser plates, while, also, those halves of the waves which are of the same polarity are alone used, the others being discarded or left on open circuit. The apparatus may be driven by power, in which case the electric motor becomes a dynamo, exciting its own field and supplying alternating current to the primary of the step-up transformer, or suitable alternating currents may drive it as a synchronous motor. Such a machine, run by continuous currents and having only eleven plates, gives sparks between its terminals over twelve inches long in rapid succession. It can be built cheaply, and is a highly instructive machine from the transformations it illustrates.

The machine is also arranged by the addition of a simple attachment, so that it may be used to charge insulated bodies, or to charge Leyden-jar condensers or the like, replacing the ordinary static machines. It might, in fact, be used to charge a second range of condenser plates in another rheostatic machine to a potential of 100,000 volts, for example. These, after coupling in series or cascade, might be made to yield potentials beyond any thus far obtained.

The interest in such experimental ap-

paratus and the results obtained come largely from the apparent ability to secure a representation of the effects of lightning discharges upon a moderate scale, and the possibility of studying the action of air and other gases, as well as liquids and solids, at varying temperatures and pressures under high electric stresses. Broadly considered, however, the similarity of the effects to those produced in a thunder cloud is more apparent than real. The globules of of water constituting the electrified cloud do not possess charges of millions of volts potential, the effects of which are seen in the stroke of lightning. The individual globules may possess only a moderate charge. When, however, they are massed together in a large extent of cloud the virtual potential of the cloud as a whole, with respect to the earth, may be enormous, though no part of the cloud possesses it. The cloud mass not being a conductor, its charge cannot reside upon its outer surface or upon its lower surface nearest the earth, as with a large insulated conductor. The charge, in fact, exists throughout the mass, each globule of water suspended in the air having its small effect upon the total result.

When the cloud discharges, the main spark branches within and through the cloud mass in many directions. The discharge can at best be only a very partial one, from the nature of the case. These are conditions which are certainly not represented in our experimental production of high-potential phenomenon, except perhaps upon a very small scale in the electrified steam from Armstrong's hydroelectric machine, a type of apparatus now almost obsolete. Yet if we wish to reproduce, as nearly as possible upon a small scale, the conditions of the thunder cloud, we shall be compelled to again resort to it. In volcanic eruptions similar actions doubtless occur and give rise to the thunder clouds which often surround the gases sent out from the crater.

Considering, then, that the conditions in the thunder cloud are so different from those in our experiments with high potentials, we can easily understand that the study of lightning phenomena may present problems difficult to solve. Two forms at least of lightning discharge are quite unknown in the laboratory—namely, globular lightning and bead lightning, the latter the more rare of the two. Personally I cannot doubt the existence of both of these rare forms of electric discharge, having received detailed accounts from eye witnesses. On one occasion, while observing a thunder storm, I narrowly missed seeing the phenomenon of globular lightning, though a friend who was present, looking in the opposite direction, saw it. The explosion, however, was heard, and it consisted of a single detonation like the firing of a cannon. According to the testimony of an intelligent eye-witness, who described the rare phenomenon of bead lightning within an hour after it had been seen, it is a very beautiful luminous appearance like a string of beads hung in a cloud, the beads being somewhat elliptical and the ends of their axes in the line of their discharge being colored red and purple respectively. This peculiar appearance, not at any time dazzlingly bright, persisted for a few seconds while fading gradually.

Again, our knowledge of the aurora is not as yet much more definite or precise than it is in regard to the obscure forms of lightning alluded to above. Whether these phenomena will ever be brought within the field of research by experimental methods is an open question.

The endeavor in the foregoing rather disconnected statements has been to indicate directions in which the field of experiment may be extended and to emphasize the fact that research must be carried on by extension of limits, necessitating more liberal en-

dowment of research laboratories. I have tried to make it clear that the physicist must avail himself of the powers and energies set in play in the larger industrial enterprises, and finally that the field of possible exploration in physics by experimental methods has its natural boundaries, outside of which our advances in knowledge must be derived from a study of celestial bodies.

The riddle of gravitation is yet to be solved. This all-permeating force must be connected with other forcec and other properties of matter. It will be a delicate task, indeed, for the total attraction between very large masses closely adjacent, aside from the earth's attraction, is very small.

Scientific facts are of little value in themselves. Their significance is their bearing upon other facts, enabling us to generalize and so to discover principles, just as the accurate measurement of the position of a star may be without value in itself, but in relation to other similar measurements of other stars may become the means of discovering their proper motions. We refine our instruments; we render more trustworthy our means of observation; we extend our range of experimental inquiry, and thus lay the foundation for the future work, with the full knowledge that, although our researches cannot extend beyond certain limits, the field itself is, even within those limits, inexhaustible.

ELIHU THOMSON.

PHOSPHORESCENT SUBSTANCES AT LIQUID-AIR TEMPERATURES.

A RECENT number of the *Philosophical Magazine** contains a paper 'On Phosphorescence,' by Herbert Jackson, which was delivered before the meeting of the British Association at Bristol, September 12, 1898, a portion of the paper being devoted to a review of the results obtained in researches relating to phosphorescent phenomena. It is evident from the paper that considerable investigation has been undertaken to ascertain the effects of high temperatures on phosphorescent substances, but that comparatively little has been done towards learning the behavior of the latter at very low temperatures, such as are obtained by the use of liquefied air. It is stated, however, in the paper referred to, that, "Professor Dewar has shown that great reduction of the temperature will cause phosphorescence to linger for a considerable time in many substances which had hitherto been considered as practically non-phosphorescent." This in particular refers to the phosphorescence produced in certain substances when exposed to light while at a temperature near that of liquefied air. Ivory, paper, and various other materials show phosphorescence under such conditions, but little or none at normal temperatures (20° C.).

Professor Dewar has found also that when a phosphorescent substance is excited by light at a normal temperature and then immersed in liquefied air the phosphorescent discharge is practically suspended, and continues so while the substance remains at the low temperature. August and Louis Lumiere have recently published a note in the *Comptes rendus*, CXXVIII., No. 9, 1899, p. 549, 'Influence des températures très basses sur la phosphorescence,' to which reference will be made presently.

The results obtained in some experiments made by the writer on the effect of liquid-air temperatures on phosphorescent substances are given below. These experiments were already completed when it was learned that the above-mentioned note in the *Comptes rendus* had been published. They were as follows:

Balmain's luminous paint, which is strongly phosphorescent at normal temperatures, was subjected to a very low temperature by the use of liquefied air, boiling

* *Phil. Mag.*, Lon. 46, 281, p. 402, Sept., 1898.

under atmospheric pressure. The sun and the electric arc were both used to excite phosphorescence in this substance. The temperature of the liquefied air employed, from experiments made with a platinum resistance thermometer, was determined to be approximately —188° C. It varied several degrees during the experiments, on account of changes in the composition of the liquefied air, due to ebullition.

Balmain's paint is a polysulphide of calcium mixed with varnish. The former is prepared by a secret process, but probably produced by mixing the powdered shells of certain shellfish with sulphur, and calcining these together at a high temperature in a closed crucible.

A number of experiments were performed with Balmain's paint at about 20 C. in order to ascertain the properties of this particular substance at ordinary temperatures.

Test cards of the phosphorescent material were prepared for the experiments in the following manner:

A sheet of cardboard was covered with dead black paper and a portion of it coated as evenly as possible with the luminous paint; it was then cut into cards, each having a surface approximately 4 x 5 centimeters coated with paint. Test plates of thin sheet iron were also coated with the same substance.

The preliminary experiments, at normal temperatures (20° C.), showed: (1) that Balmain's paint, exposed for a few seconds to the sun or the electric arc, gave bright violet phosphorescence when removed immediately away from strong light, and that after being in the dark for three hours its luminosity was so faint as to be hardly visible; (2) that a very slight elevation of temperature, a few degrees, caused a perceptible brightening of the phosphorescent surface, and that when the temperature of the substance was lowered, its luminosity was lessened in a corresponding manner; (3) that the brightening of the phosphorescent surface caused by the application of heat was merely a rapid discharge of the phosphorescent energy; (4) that a test card of luminous paint exposed to sunlight and then placed in darkness for three weeks showed phosphorescence when heated to about 300 degrees C.; (5) that the part of the solar spectrum producing phosphorescence in the substance employed was practically entirely that towards the violet end; (6) that the infra-red rays of the solar spectrum falling on a phosphorescent surface rapidly discharged the phosphorescence.

Those who have investigated this subject are no doubt familiar with these facts, but they are mentioned here for the purpose of comparison with the observations made at low temperatures.

1. When a card covered with Balmain's paint was exposed to strong sunlight, taken into a dark room, and then immersed in liquified air (about 188° below zero C.) its phosphorescence was so reduced as to appear perfectly destroyed. When, however, this card was allowed to warm up gradually to the temperature of the room (20° C.) the phosphorescence again became active, being almost as bright as before immersion in liquefied air.

2. A phosphorescent card treated in the manner just described, except rapidly warmed to the temperature of the room (from—188° C.) by immersion in water at 20° C., was then compared with a card having had the same exposure to sunlight but not placed in liquid air. It was observed that the two cards presented little difference in luminosity, showing that the temporary reduction of the temperature of the one placed in liquid air had not resulted in an appreciable change in its phosphorescent energy. The card which had been in the liquid air was slightly the brighter of the two. This was to be expected, because the

discharge of its phosphorescence had been temporarily checked by the cold liquid bath, while the phosphorescence of the other card, which was at 20° C. throughout the experiment, was being discharged continuously; the latter, therefore, was slightly more exhausted.

3. Two cards similarly coated with Balmain's paint and which had been placed away from light for several days were exposed to the sun; one ('A') in the ordinary manner, and the other ('B') exposed while immersed in liquid air.

The cards were brought to the dark-room, and the one ('A'), that had not been subjected to the low temperatare, was immediately immersed in liquid air.

Both cards were then taken out of the cold bath and allowed to warm up gradually to the temperature of the dark room. Almost immediately card 'B', which had been exposed to sunlight while in liquid air, showed phosphorescence, appearing comparatively bright by the time the other card ('A') became luminous; the latter, however, getting rapidly brighter soon phosphoresced more strongly than the card 'B', which was exposed to the sun while immersed in liquid air.

This experiment was repeated many times with always the same result.

Several times the test card which had been exposed to the sun while in liquid air showed faint phosphorescence when still in the cold bath. On employing an electric arc, however, the Balmain's paint was made to phosphoresce much more strongly while at the temperature of liquid air than when using sunlight.

On repeating the experiment with two stop-watches it was found that the card 'B', exposed to the sun while in liquid air, required not more than 5 to 10 seconds to show phosphorescence distinctly, when allowed to warm up gradually to 20° C. in the dark room. The card 'A', simply exposed to the sun and immersed in liquid air in the dark, required 40 seconds as the average time to show luminosity.

In order to obtain an approximate value for the temperature corresponding to this time of 40 seconds, small sticks of wood, weighted with lead, were frozen to the test card with alcohol and afterwards with mercury. The cards were placed in liquid air and then allowed to warm up to the room temperature exactly as in the experiments on phosphorescence. The sticks frozen with alcohol fell from the test cards in 17 seconds, a value found by ten determinations, showing little variation from the mean. The sticks frozen with mercury fell from the card in 90 seconds. From this it was obvious that the 40 seconds required for the phosphorescence to become visible in the experiment mentioned above with card 'A' corresponded to a temperature between the melting point of alcohol and that of mercury, or a temperature of from 75° to 100° below zero C. Only this rough approximation of temperature was made because a more accurate determination would have had no general significance. This is evident when it is remembered that the phosphorescing power of different samples of the same material, the intensity of the light employed to excite a phosphorescent substance, and the sensitiveness of the eyes of different observers, all of which determine temperature values like that under consideration, are factors which are subject to considerable variation.

The results obtained in the foregoing experiments are in agreement with those published in the *Comptes rendus* by A. and L. Lumiere, already referred to. In the experiments of these investigators the electric arc was employed to excite phosphorescence in sulphides of calcium and zinc at normal temperatures; the phosphorescence produced was found to become invisible when these substances were cooled down from

normal temperatures to from —45° to —55° C.

When magnesium light was used as the exciting agent, the resulting phosphorescence did not become invisible until the substances were cooled to between —70° and —90° C. These temperature values might have been considerably different if other samples of the same phosphorescent materials had been used.

Other experiments by the undersigned were as follows:

4. A test card coated with phosphorescent paint was placed under a color screen, composed of strips of red, yellow, green and blue, transparent celluloid. The paint was then exposed to sunlight, both at 20° C. and at the temperature of liquefied air (—188° C.). The phosphorescence produced at 20° C. was more intense than at —188° C., but in the two cases the relative effects of the colored light appeared to be the same, blue light giving strong phosphorescence, while red light produced very little.

5. A test card ('A') of phosphorescent paint was exposed to sunlight for two minutes, the first at 20° C., the second at —188° C. (in liquid air). The card was then taken into a dark room, where it became visibly phosphorescent when it had risen in temperature a few degrees above —188° C. When this card was warmed to 20° C. it appeared equal in luminosity to a card 'B' that had been exposed one minute to the sun while in liquid air and then warmed to 20° C. Thus it appeared that the previous additional exposure to the sun of the paint on card 'A' of one minute at 20° C. did not appreciably increase the intensity of its phosphorescence.

The results of these experiments, as well as the observations of other writers mentioned above, indicate that the principal effects of very low temperatures on phorphorescence are as follows:

A. That the reduction of the temperature of a phosphorescing substance is accompanied by a corresponding decrease in the phosphorescent discharge.

B. That very low temperatures cause phosphorescence to linger long enough to be readily observed in a number of substances that are not visibly phosphorescent at normal and high temperatures.

C. That the production of phosphorescence in a phosphorescent substance is less when excitation occurs at low temperatures than when it takes place at high temperatures, other conditions being the same.

Furthermore, it appears as if, for a certain phosphorescent substance, different rates of phosphorescent discharge correspond to definite degrees of temperature, other conditions being the same, and that when an excited phosphorescent substance has been reduced to a very low temperature (even to —200° C.), so as to show no phosphorescence, there is still some phosphorescent activity present. In such a case, however, the eye is not sensitive enough to detect the existing feeble luminosity.

A number of common substances show marked phosphorescence when reduced to the temperature of liquid air and then exposed to strong light; besides ivory and paper, already mentioned, are gum arabic, cotton wool, starch, white glue, celluloid and kid-skin.

The colors produced in the phosphorescence of these substances were observed to be as follows:

Gum arabic; decided light blue.
Ivory; bluish white. (Some pieces greenish.)
Cotton wool; bluish green,
Starch; yellowish green.
Paper; yellowish green.
White glue; greenish yellow.
Celluloid; greenish yellow.
Kid-skin (glove); decided green.

All of these substances were placed in filtered liquid air, exposed to an arc light,

and then examined in the dark. They remained bright at least half a minute, when kept at a low temperature after excitation.

Calcium tungstate was found to give phosphorescence decidedly green in color, after being exposed to an arc light, while at the temperature of liquid air, and then examined in the dark. When this substance is subjected to the influence of Röntgen rays the fluorescence produced appears white.

Experiments on the present subject will be continued when more liquefied air is procured.

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SCIENTIFIC BOOKS.

Synopsis of the Recent and Tertiary Leptonacea of North America and the West Indies. [Proceedings U. S. National Museum, Vol. XXI., Pages 873–897 (with Plates LXXXVII., LXXXVIII.) 1899.] By WILLIAM H. DALL.

The Mollusca of Funafuti. Part I., *Gastropoda.* [Memoirs of Australian Museum III., Part 7, March 6, 1899.] By CHARLES HEDLEY.

In the first of these two very interesting and valuable additions to the literature of Malacology, Dr. Dall gives descriptions and figures of eighteen (18) new species and conveniently arranges all the known forms in the three lists —East Coast, West Coast, and Tertiary—showing that of the thirty-five east coast species enumerated, there are but two (*Kellia suborbicularis* and *Turtonia minuta*) which are found also on the west coast, the *Lasæa rubra* from Bermuda proving to be a distinct species. But four (*Kellia suborbicularis*, *Mysella planulata*, *Turtonia minuta* and *Aligena elevata*) are also found among the fossils. Many interesting changes are made in the synonymy, especially in the species from northeastern waters, some alteration in the name being made in every instance. They require the careful consideration of students interested in these small forms.

The combining of the recent genus *Kelliopsis* Verrill and Bush (1898) with the fossil genus *Aligena* H. C. Lea (1846) is unquestionably correct, but it is not made clear why *Abra æquata* Conrad (1843) (p. 877) is given as the type, and the two species (*A. striata* and *A. lævis*) described and figured by Lea fail to be mentioned even in the list of Tertiary species.

It is very doubtful if the combining of the genus *Mysella* Angas (1877) with *Montacuta* Verrill and Bush (1898) (the universally accepted interpretation of this genus), and the new interpretation of *Montacuta* Turton (1822), should remain unquestioned (p. 881).

In *Mysella* the hinge, the most important character, is described by Angas as consisting, in one valve, of a small, diverging, triangular cartilage-pit, close to which is a single, small, diverging, subcircular, flattened cardinal tooth; and, in the opposite valve, of two, thin, short, horizontal, lateral processes (P. Z. S. London, p. 176, 1877). The figures given of the only species (*M. anomala*) are entirely too small for accurate study and apparently resemble those of *M. substriata* as much as those of *M. bidentata*. It would seem improbable, however, that Angas could have failed to recognize the relationship of the Australian form to either or both of these well-known species. Probably with a more critical study of the specimen itself and with more material new points would be brought out, especially as it was placed by Angas between *Ervilia* and *Cytherea*, well separated from *Kellia*. In the species which Dr. Dall refers to *Montacuta* Turton, he describes and figures the hinge as having a prominent cardinal in each valve; the lamellæ obsolete; sockets for the resilium thickened and raised above the inner surface of the valve (*M. floridanum*, p. 893). The other species are similar to this, but he places the *Tellimya ferruginosa* Verrill (*non* Montagu) = *percompressa* Dall (p. 894) with them.

In an interior view of *percompressa*, the valves united, the dark brown resilium is somewhat triangular in form and lies underneath the beaks, fastened by its thin, inner edge to the sunken sockets which lie underneath the thickened posterior hinge-margin; its outer or upper edge is thick and broad, and bears an inconspicuous, thin, white ossicle. In one valve the anterior hinge-margin is thicker than in the

other and bears on its inner end a prominent, slender, somewhat curved tooth, having a blunt, concave end; in the opposite valve the hinge-margin is narrower, and the tooth, of similar form, is but little raised; a portion of the small, black or darker brown, external ligament, which separates the beaks and extends a little way on each side, passes between the valves and lies between these teeth, firmly attached to their concave ends. The dorsal margin is further attached, by the extension of the thin epidermis, for a considerable distance both before and behind the beaks.

The *Tellimya* Brown (1827) in its extended sense is synonymous with *Montacuta* Turton (1822). Brown divided his genus into three sections; the first included *suborbicularis* Montagu (the type of *Kellia* Turton (1822)); the second included *elliptica* Brown and *glabra* Brown, both = *ferruginosa* (Montagu), (recognized as the type of *Tellimya*, taken in a restricted sense), *bidentata* (Mont.) and *substriata* (Mont.) (type of *Montacuta* given by Woodward, 1851). Of the third section no examples were given. The hinge of this last species is carefully described by Jeffreys (Br. C., II., p. 206, 1863) as follows: "Cartilage yellowish-brown and semicylindrical, clasping the hinge-line on the posterior side of the beaks; hinge-plate short and narrow but strong, not deeply excavated in the middle; teeth triangular and pointed, that on the anterior side in each valve being larger than the other; the teeth in one valve lock into sockets in the other, but not in the corresponding valve of every specimen, it apparently being indifferent whether the right or left valve contains the more prominent teeth or sockets."

H. and A. Adams (1858) separated the two genera, placing *substriata* as the only example of *Montacuta* and both *bidentata* and *ferruginosa* as examples of *Tellimya*, but the definitions give no distinguishing characters. Sars in 1878 used *Tellimya* for *ferruginosa* (Mont.) and described and figured two new species, *nivea* and *ovalis*, and placed *bidentata* Mont., and *substriata* Mont., etc., under *Montacuta*. Professor Verrill also used *Tellimya* for the American form (*percompressa* Dall), thought to be a variation of the English *ferruginosa*, and gave in Trans. Conn. Acad., Vol. VI., p. 225, 1884, description and figures of the animal. The animal of the true *ferruginosa* was first described by J. Alder (Ann. Mag. Nat. Hist., p. 210, 1850). That Dr. Dall's interpretation of *Montacuta* appears synonymous with this is the probable reason for its not being used in his article. But that *substriata* and *ferruginosa* will prove generically related needs careful consideration, especially as no external ligament has been mentioned as found in *substriata*, and only separate valves have been found of each of Dr. Dall's new species.

Jeffreys (Proc. Zoöl. Soc., London, p. 696, 1881) proposed for the species described and figured by G. O. Sars as *Tellimya ovalis* the new name *Decipula ovata*; the new generic name, because *Tellimya* is a synonym of *Montacuta*; the specific, because he disapproved of the meaning of *ovalis*. As Sars' excellent figures show nothing that would generically separate *ovalis* from *ferruginosa*, *Decipula* becomes synonymous with *Tellimya*, but if the latter is discarded the former would have to be retained for this group. Jeffreys also made Sars' second species, *nivea*, a variety of *ferruginosa*. [See also Monterosato, Bull. Soc. Mal. Italian, Vol. VI., pp. 57–8, 1880.]

Montacuta planulata Stimpson (1851) was used by Professor Verrill in his Vineyard Sound Report (1874), but the great variations in size and relative thinness of texture in the American shells and their marked resemblances to a small series of English specimens led to the conclusion that in a larger series the same variations might be found and the English name *bidentata* was adopted. Dr. Dall finds that such variations do not exist, and again restores Stimpson's name (p. 890). This is not the *Lasæa planulata* Verrill (1879, Check-list) which is a large species of the same genus, measuring 8.5 mm. in length and about 7.5 mm. in height. It was dredged by the U. S. F. C. in Halifax harbor, in eighteen fathoms, 1877. It is thin, of delicate texture, covered with a conspicuous dirty-brown epidermis; the hinge-teeth are unequal in length and strongly resemble the figure given by Dr. Dall, of *Mysella Mölleri* (Hölbol) Mörch, so that there is no doubt that it is the same species.

It should be further noticed that in nearly every instance where the name of an Eastern Atlantic species has been adopted for species of our Western Atlantic fauna, even in those from deep water, Dr. Dall has either changed it or given additional varietal names, sometimes only for the very insufficient reason of differences in size, as in the instance of *Kellia suborbicularis* (Mont.) (p. 889), where only two specimens have been recorded from the coast: Thompson's *Gouldii* from New Bedford harbor and one from Massachusetts Bay, off Salem, U. S. F. C. (Verrill and Bush, 1898).

Such great dissimilarities exist in the hinges of the five figured species referred to the genus *Erycina* as to render it improbable that they can be retained in so close generic relation.

All these doubtful points will doubtless be satisfactorily adjusted by Dr. Dall in his more extended discussion of the subject, which is to appear in the Trans. Wag. Inst., Philadelphia, Vol. III., Pt. 5.

The *Lasæa rubra* Montagu quoted from Bermuda (p. 876) was found there abundantly by Professor Verrill and party, 1898. Compared with specimens from Guernsey it is found to have a much more swollen form with very large, swollen umbos, and attains twice the size of any of our numerous English examples. In one valve, anterior to the beak, there is a short, deep, socket, not sunken below the surface of the hinge-margin, but formed by two thin, triangular, raised teeth, nearly parallel—the outer one next to, and parallel with, the dorsal margin, and the inner one, much longer, diverging from the beak and curving outward from the inner edge of the moderately wide margin, the highest point of each being near the distal end. In front of this is a small, little prominent tooth on the inner edge of the hinge-margin, directly under, but separated from, the beak. There is also a similar socket, a considerable distance behind the beak, but it is longer and the two teeth are less triangular and but little raised, the upper or outer one scarcely discernible; within, and somewhat in front of this, separated and diverging from it, and running backward from the beak, is the sunken socket, to which a long, conspicuous, white resilium is attached. In other words, the hinge-margin broadens out distally, forming a triangular-shaped ledge at the side of this inner tooth, which has a concave side in which the resilium lies. In the English *rubra* the resilium is amber color and the teeth are not so strongly developed as in Bermuda specimens of the same size. In the opposite valve of the former there are three prominent teeth, the lateral ones well separated from the dorsal margin, which fit into these sockets and a corresponding resilium-pit. This distinct species may take the name *Lasæa Bermudensis*, sp. nov.

In the second article Mr. Hedley gives in his preface an interesting account of the atoll of Funafuti and the positions and conditions under which the various forms of mollusks are found, calling attention to the peculiarity in their lack of development, they being of smaller size than the representatives of the species from other localities. He also calls attention to the great difficulties encountered in preparing his article, owing to the great paucity of descriptive material.

Of the two-hundred and ninety-seven species, besides varieties, enumerated, about thirty-seven are described as new. Three new genera are also introduced (*Obtortio*, p. 412, type *Rissoa pyrrhacme* Melvill and Standen; *Cotumax*, p. 436, type *C. decollatus* sp. nov.; and *Thetidos*, p. 472, type *T. morsura* sp. nov.). The first is probably erroneously referred to Turbonillidæ, as there is nothing in the description or figure to suggest such a relation, so that a careful study of the animal is needed before such a question can be correctly determined. The second is placed with the Cerithiidæ, its nearest ally, *Cerithiopsis;* while the third is an addition to the Mangilliinæ but seems synonymous with *Nassarina* Dall (1889). Although the few figures given are unfortunately crude and coarse, they are of sufficiently large size to bring out the characters necessary for identification.

KATHARINE J. BUSH.

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'Über die thermo- und piezo-elektrischen Eigenschaften der Krystalle des ameisensauren Baryts, Bleioxyds, Strontians und Kalkes, des Schwefelsauren Kalis, des Glycocolls, Taurins und

Quercits.' W. G. Hankel, Abhand. der math phys. Classes der K. Sächs. Gesell. der Wissen. Bd. XXIV. Pp. 471–496.

In this, the twenty-first paper by Professor Hankel describing his electrical investigations of crystals, the object, as before, is to determine the character and relative intensities of the electric charges developed at different parts of the crystals under the influence of temperature change or of pressure. From this distribution of the positive and negative charges conclusions can be drawn as to the true structural symmetry of the crystals. The methods were presumably those followed in previous investigations, as they are not described.

A. J. M.

SCIENTIFIC JOURNALS AND ARTICLES.

The *Physical Review* for August contains the following articles:

'The Specific Heat of Solutions which are not Electrolytes,' by William Francis Magie.

'An Interferometer Study of Radiations in a Magnetic Field,' II., by John C. Shedd.

'The Effect of Magnetization upon the Elasticity of Rods,' by J. S. Stevens and H. G. Dorsey.

'On Freezing and Boiling Water Simultaneously,' by R. W. Quick.

Bird Lore for August opens with an article by R. Kearton, one of the most successful of the many photographers of wild animals, on 'Photographing Shy Wild Birds and Beasts at Home,' in which are explained some of the devices used by the Kearton Brothers. 'Two Nova Scotia Photographs,' by C. Will. Beebe, show in a very beautifully surrounded nest of Junco and a sleeping nighthawk. 'In the Spartina with the Swallows,' by O. Widmann, treats of a vast Western swallow roost in this writer's usual charming style and is accompanied by some interesting views. Bradford Torrey tells of 'Watching the Bittern Pump' and the various 'departments' are well filled, among the articles being a 'Round Robin' signed by well-known ornithologists, entitled 'Hints to Young Students' and justly deprecating the wholesale slaughter of birds and collecting of eggs under the impression that this alone is ornithology.

DISCUSSION AND CORRESPONDENCE.

ON GRADUATE STUDY.

In the article, in Science for August 4th, 'Doctorates Conferred by American Universities,' in which you speak of the comparatively small number of university doctorates in the humanities, is found the following statement: "Our educational system is largely based on the study of language, and in view of the great number of teachers required it appears that they are satisfied with a less adequate education than is the case in the sciences." Every suggestion that looks toward improvement in the preparation of teachers, especially of the teachers in secondary schools, who seem most vulnerable in qualification in languages, should be warmly welcomed, but I am sure, however, that not all university teachers will agree with the conclusion quoted above.

It is certainly true, as your comparative table shows, that in American universities more candidates seek the degree of Doctor of Philosophy in the sciences than in the humanities, but it does not, therefore, necessarily follow that the persons who are engaged in teaching the humanities in our better colleges and universities 'are satisfied with a less adequate education' than is the case with their colleagues in the sciences, nor should a teacher's qualifications be measured by the number of degrees he possesses. As is well understood, language teachers often feel that they can do graduate work to better advantage in Europe, where they are constantly surrounded, as it were, by the very things they are studying; in fact, some American institutions decline to consider the applications of candidates for positions in French and German who have not studied abroad. These facts, and the additional fact that we now have better scientific laboratories in this country than was formerly the case, would perhaps partly explain the inequality in the number of doctorates conferred by American universities in the humanities and in the sciences. In this connection it is interesting to note that of the American students engaged in the study of these subjects at the University of Berlin during the summer semester of 1897 (I have no later statistics at hand) nearly twice as many were study-

ing the humanities as were matriculated in the sciences, or by actual count 63 % against 37 %.

CHARLES BUNDY WILSON.

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NOTES ON THE NOMENCLATURE OF SOME NORTH AMERICAN FOSSIL VERTEBRATES.

IN *Palæontological Bulletin* No. 16, p. 5, published August, 1873, Professor E. D. Cope described a new genus of rodents which he called *Gymnoptychus*. Of this genus he described at the same time four species, viz.: *chrysodon*, *nasutus*, *trilophus* and *minutus*. Later in a paper published in the *Seventh Annual Report of the U. S. Geological and Geographical Survey of the Territories*, on page 477, Professor Cope shows that he had determined that his *G. chrysodon* was identical with *Ischyromys typus*, described by Dr. Leidy in 1856. Accordingly *G. chrysodon* is recorded as a synonym of *I. typus*, while *minutus* and *trilophus* are retained under *Gymnoptychus*, the form *nasutus* being regarded as a probable synonym of *trilophus*. The same disposition is made of the species in Cope's *Vertebrata of the Tertiary Formations of the West*, except that *nasutus* is there made a synonym of *minutus*. It is evident that an error in nomenclature has been committed. Professor Cope nowhere definitely states which of his species he originally regarded as the type of *Gymnoptychus;* but, considering the way in which the species *chrysodon* is associated with the new genus *Gymnoptychus* and Professor Cope's practice in other cases, we are justified in believing that he regarded *chrysodon* as the type. But if this conclusion is contested there is indubitable evidence. The characters of *Gymnoptychus* are derived from the dentition of both upper and lower jaws; and *chrysodon* was the only species of which he possessed both mandible and maxilla. It must, therefore, be regarded as the type of *Gymnoptychus*. Hence, when *chrysodon* was proved to be identical with *Ischyromys typus*, *Gymnoptychus* became a synonym of *Ischyromys*, and was no longer available as a generic name for the species which had been associated with it. These require a new generic name, and I therefore propose *Adjidaumo*, having for its type Cope's *Gymnoptychus minutus*. *Adjidaumo* is taken from Longfellow's *Hiawatha*. The known species are *A. minutus* and *A trilophus*.

MR. E. S. RIGGS has recently proposed in Field Col. Mus., Geol., Vol. I., p. 183, a new generic name, *Protogaulus*, for the reception of *Meniscomys hippodus*, since he considers that the species is not congeneric with the others which have hitherto been associated with it. This new genus Mr. Riggs arranges in the family *Mylagaulidæ*. Even if Mr. Riggs' views regarding the generic distinctness of *hippodus* and regarding its family relationships prove to be correct, he has proceeded in an improper way to express his conclusions. The type of the genus *Meniscomys* is the species *hippodus*, and in this genus it must remain, unless it can be shown either that *Meniscomys* is preoccupied or that it is a synonym of some earlier genus. *Hippodus* is provided for; it is the other species which are deprived of generic name by the removal of *hippodus*. They, however, may find lodgment under Marsh's *Allomys*. As the matter stands, *Protogaulus* is merely a synonym of *Meniscomys*, and both possibly synonyms of *Allomys*.

IN the *American Journal of Science*, 1871, Vol. II., p. 125, Professor Marsh described, from the Bridger Eocene of Wyoming, a fossil carnivore which he called *Canis montanus*. This name, however, was preoccupied, having been employed in 1836 by Pearson. In the *Journal of the Asiatic Society of Bengal*, Vol. V., p. 313, he describes a fox which he called *Canis vulpes montana*. Although this animal is regarded by some as at most a subspecies of *Canis* (*Vulpes*) *alopex*, and although Professor Marsh's species probably belongs to a different genus, nevertheless, the latter species is shut out from the enjoyment of the name *montanus*. I shall apply to it the name *Canis? marshii*.

IT is necessary to call the attention of paleontologists to the fact that the genus *Hypotemnodon* can not be employed for the two species which have been arranged under it. *Hypotemnodon* was proposed in 1894, by Dr. John Eyerman, in the *American Geologist*, Vol. XIV., p. 320, the type species being Professor Cope's *Temnocyon coryphæus*. But already, in 1890, in an article entitled 'The Dogs of the American Miocene,' published in the *Princeton College Bulletin*, Vol.

II., p. 37, Dr. W. B. Scott established the genus *Mesocyon*, basing it on the same species *coryphæus*. Dr. Scott seems to have afterwards forgotten his genus, since he employed Eyerman's name. Indeed, all paleontologists who have had occasion to mention the genus have called it *Hypotemnodon*. It is obvious, however, that it must yield to *Mesocyon*.

In 1865, in Proceed. Acad. Nat. Sciences of Philadelphia, p. 90, Dr. Leidy described, from the Eocene of South Dakota, a carnivore which he called *Amphicyon gracilis*. Unfortunately for his species, Pomel had, as early as 1847, employed the same name for a fossil carnivore found in Europe. Cope in 1884, in his Vertebrata of the Tertiary Formations of the West, p. 916, made Leidy's name a synonym of *Galecynus gregarius*. Scott and Osborn in 1887, in a paper in the *Bulletin of the Museum Comp. Zoology*, Harvard, Vol. XIII., p. 152, speak of it as a distinct species under the name *Cynodictis gracilis*. Matthew recently, in *Bulletin of the American Museum*, Vol. XII., p. 54, records it as an 'invalid species' and apparently as a synonym of *Cynodictis lippincottianus*. When those disagree who have access to the type specimens and to abundant materials belonging to related forms, it is evident that the last word has not been said. Until it can be determined with some degree of unanimlty where Leidy's specimens belong, it will be better to keep them to themselves under a distinct name. Furthermore, the possibility exists that the discovery of additional materials will prove Leidy's form to be a good species. Pending this settlement of the question I propose to call the *Amphicyon gracilis* of Leidy *Cynodictis hylactor*. The specific name is that of one of Actæon's dogs.

O. P. Hay.

U. S. National Museum, July 27, 1899.

THE PROPER NAME OF THE POLAR BEAR.

The technical name of the Polar Bear as usually mentioned is *Thalarctos maritimus* (Linn.), the reference being Systema Natura, X., 1758, p. 47. In looking up this reference I find it is simply mentioned under *Ursus arctos*, as follows: 'Ursus maritimus albus major arcticus'; with a reference to Martën's Spitzbergen, and concluding with a note doubting the specific distinctness of this bear. A question as to the value of this reference was referred to several noted authorities on the Mammalia, whose answer did not sustain the reference, and induced me to examine the case closer. The next date when any mention of the Polar Bear was made was 1776, when Müller and Pallas each gave it a name. Müller in his Zoologiæ Danicæ Prodromus, etc., p. 3, refers to it as a variety of *U. arctos*, calling it *U. albus*, but giving only a reference to Martën's Spitzbergen, and a short note on its habitat. Pallas, in his Reise, III., bh. 2, p. 691, describes this species as *U. marinus*, with a good diagnosis, which proves he knew the animal very well. As the name of Pallas is undoubtedly the best, being accompanied by a good description, therefore the name of the Polar Bear should be *Thalarctos marinus* (Pallas). The reference is Reise, III., bh. 2, p. 691, 1776.

James A. G. Rehn.

Academy of Natural Sciences, Philadelphia, August 7, 1899.

THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.

To the Editor of Science: A few days after contributing to your esteemed journal my remarks upon the bibliographical methods proposed for the Catalogue of Scientific Papers I received a report of a committee of Dutch scientists, whose conclusions are diametrically opposed in certain points to the opinions which I expressed. Impartiality requires that I should not pass this criticism unnoticed.

Let me translate from the French text: "The adoption of the Dewey Decimal Classification having been favored by certain persons, we wish to state our opinion in regard to this system. This opinion is very unfavorable. In our opinion the adoption of the system would lead to the failure of the enterprise.

"Our conviction in this matter is based upon the faulty manner in which the system has been worked out for various sciences in the 'Decimal Classification and Relative Index' of Mr. Dewey (1894, Library Bureau, 146 Franklin Street, Boston; 21 Bloomsbury Street, London).

It is possible that a classification such as is here proposed may serve for arranging a library containing principally popular science works or pedagogy, but for the end proposed by the International Catalogue it seems to us inapplicable. In regard to certain sciences, notably mathematics, physics, astronomy, it seems to us scarcely possible that there can exist in this regard a serious difference of opinion among persons really competent to judge.

"Furthermore, we reproach the system as worked out by Dewey with being inelastic. In view of the very restricted number of places left vacant, the addition of new subjects of a fundamental character can soon be accomplished only by very artificial means, and, moreover, it would require the use of a disproportinate number of the former figures."

On reading this report one can hardly fail to be struck with the emphasis that has been laid upon the book that has been used as an authority, pains being taken to give even the street number of the firm selling the work in Boston and in London. This emphasis is, of course, in part, due to the fact that the committee wished to prove that its criticism was not made without examining the 'Decimal Classification' of Dewey, a neglect which has been admitted by certain other critics. But to those who have followed the matter closely it will be apparent that this assertion has a deeper meaning. It is a frank declaration that the committee declines to examine the application of the decimal system to card bibliographies. Dewey, as is well known, never proposed the use of his system for bibliographies. It is an application which I believe was first made by Mr. Pickford Mann, but which has since found wide extension largely in consequence of the effort of the International Institute of Bibliography in Brussels. Now, this statement is intended as a disavowal of these applications. Such a proceeding is manifestly unfair. What should we say of a person who should insist upon using a work dealing with electric lighting as an authority for judging the possibility of utilizing electricity for telegraphy? The Brussels Institute took the decimal system, expanded certain parts according to the principles expounded by Dewey, added a few distinctive signs, such as the colon and the parenthesis, and at once the system attained the extreme pliability requisite for bibliographic purposes. For library purposes pliability is a fault, a work on the Locusts of Mexico can not be duplicated under Locusts and under Mexico; but for bibliography this is a *sine qua non*. Moreover, the success or failure of the system in libraries is no valid argument respecting its use for card bibliographies. In library organization the question is whether or not a methodical arrangement of the books according to subject-matter be possible and practical. Where the decimal system has failed, it will be found to have been the strict methodical arrangement that has been found impractical. But for bibliography the arrangement by subject-matter, however difficult to attain, is essential, and *for cards* this presupposes some system such as the decimal system.*

The report states that for various sciences the system has been worked out in such a faulty manner that it seems scarcely possible for a divergence of opinion to exist. The sciences that are selected as examples differ from those mentioned in the memorandum of the Royal Society's Committee. According to the Dutch report, mathematics, astronomy and physics can not be dealt with in this way. To deny the possibility of a divergence of opinion in this regard is certainly too strong. I have laid the matter before the representatives of these sciences in Zürich, and two of them declare themselves pronounced advocates of the decimal system; the third believes it perfectly applicable. For mathematics, Professor Rudio, who has been watching the movement for a year past, feels that certain changes are necessary and pointed out the modifications necessary to bring the scheme into harmony with the Jahrbuch. It is, indeed, my conviction that the objections raised relate to the fact that the classification is conventional, not scientific. But it is easy to show that this is no valid ob-

* It is important to note that, out of over one thousand divisions used by Dewey in the part worked out by the Concilium Bibliographicum, only three have been modified. This is a sufficient answer to those who claim that the system must be totally remodeled.

jection; on the contrary, a classification embodying the latest scientific conceptions is seldom fit for bibliographical work. In the Dutch Academy of Sciences ridicule was cast upon the decimal system because physiology was made a sub-division of medicine. Scientifically it is absurd; bibliographically it is the only wise course. The literature of the past century passes insensibly over into medicine, and a system disregarding this historical fact would be extremely faulty. All attempts at a strictly scientific classification must be personal and liable to change. Most zoologists place *Limulus* with the Arachnids; bibliographically this would be folly. Arachnidologists, collecting the spiders of the various countries of the world, have not yet, at least, become so impressed with this kinship that they seek the seas for *Limulus*; while the malacologists persistently add *Limulus* to their lists of captures. The bibliographical system should correspond with the customs of authors; it is not intended to teach taxonomy.

The assertion that the decimal system is inelastic scarcely needs comment. The system was first published in 1876, with 1,000 divisons, requiring 12 pages of print; to-day by simple expansion nearly 50,000 divisions, filling 400 pages, having been added. For certain sciences the expansion has been continued still further. Indeed, there are now far more divisions in our simple zoological tables than in the entire original work. In certain trials leading up to the establishment of the final system used by the Concilium Bibliographicum the attempt was made to proceed by successive sub-divisions down to families and sub-families. In this experiment as many as a thousand new divisions were introduced at a single point in the series; it is needless to say that no inconvenience was experienced.

It is a pity to see cautious men of science make assertions like this, which have not the slightest foundation in fact. They are so plainly based upon gross misconception that one might well pass them by in silence were it not that they are liable to have weight in deciding one of the most vital questions now before the scientific world.

Herbert Haviland Field.

NOTES ON INORGANIC CHEMISTRY.

The paper read by Collie and Tickel before the Chemical Society (London) on the quadrivalence of oxygen, as shown by the probable constitution of dimethylpyrone, 'an oxygen base' has been recently noticed in this column. In this paper the authors mention that in 1888 J. F. Heyes advocated a similar view to account for such peroxides as MnO_2 and BaO_2. In the last *Chemical News* C. T. Kingzett calls attention to the fact that in a paper before Section B at the Southampton meeting of the British Association, in 1882, he reviewed the modes of formation of ozone and hydrogen peroxid, arguing for the variable valence of oxygen, and adds: "I am not aware that anyone had previously represented oxygen as a tetrad." Being present at the Southampton meeting, I remember Mr. Kingzett's paper very well; indeed, I was so much impressed with it that I have since used the formulæ $O=O^{iv}=O$ and $H_2O^{iv}=O$ in my teaching. I recall, however, that after the session one of the members remarked to me: "Kingzett is right, but there is nothing new in it; I have been teaching that for a number of years." It has long seemed strange to me that the idea of the variability of oxygen's valence has had so few advocates, especially when its position in the periodic system is considered.

In a recent number of the *Archiv der Pharmacie* a new method of detecting arsenic in fabrics is given by O. Rössler. A small piece of the goods is burned in the upper part of a Bunsen flame in a fine platinum spiral, and the arsenious oxid formed collected on the outside of a porcelain dish filled with cold water. The deposit, which is hardly visible, is moistened with silver nitrate. On subsequent fuming with ammonia the yellow precipitate of silver arsenite becomes visible, and then disappears by solution in more ammonia. No data are given as to the delicacy of the reaction, but it must be vastly inferior to Reinsch's test, except for such compounds of arsenic as are wholly insoluble in hydrochloric acid. In the case of the sulfids of arsenic Rössler's test might have a considerable value, as the quantity of arsenic present in such a yellow pigment is not small.

It was only after much experimenting that Tyndall succeeded in obtaining absolutely optically-pure air, so clear that the path of even the most intense light is invisible. In experimenting upon water Lallemand was unable to obtain optically-pure water, and was led to question its existence. Professor Spring, of Liège, has, however, succeeded in thus purifying water and an extended account of his work is given in the *Bulletin* of the Royal Academy of Belgium. That the visibility of the path of a ray of light is due to suspended matter is shown by the fact that in the optically-pure water of Spring the path is wholly invisible. Such water cannot be obtained by distillation, nor by filtration, these processes having no tendency to purify water optically, but often rather the contrary. Optically-pure water can be obtained by the action of a weak high-tension current on water which contains suspended matter, or by sedimentation of colloidal precipitates, such as aluminum, ferric, etc., hydroxids, or by filtration of the water through colloidal precipitates. Crystalline precipitates, such as calcium oxalate, have little or no effect; nor can organic liquids be purified by colloidal precipitation. Spring believes that the illumination of the water is probably not caused by the dust itself, but by minute bubbles which adhere to the dust particles. These occasion different colors, in which red and orange predominate, and hence the blue color of natural waters cannot be due to a selective absorption on the part of the suspended matter. J. L. H.

THE BRITISH MUSEUM.

A RETURN dealing with the British Museum has been issued in the form of a Blue-book. We learn from a report in the London *Times* that it contains an account of the income and expenditure of the British Museum (Special Trust Funds) for the year ended March 31, 1899; a return of the number of persons admitted to visit the Museum and the British Museum (Natural History) in each year from 1893 to 1898, both years inclusive; together with a statement of the progress made in the arrangement and description of the collections and an account of objects added to them in the year 1898.

Part VI. of the return, which gives an account of the general progress at the Museum at Bloomsbury, states that the number of visitors to the Museum in the year 1898 is the highest on record since the year 1883, amounting to a total of 612,275, as against 586,437 in 1897. The visitors on Sunday afternoons numbered 41,858, as against 37,594 in 1897. The total number of visits of students to the reading room during the year was 190,886, a slight increase on that of the previous year, which was 188,628. The daily average was 627, as against 624 in 1897. The number of volumes, etc., supplied to readers in the year was 1,397,145, as against 1,419,159 in 1897. There has been a marked increase in the number of visits of students to the several other departments other than the reading room. The total amounted to 48,214, as against 40,976 in 1897. This increase is partly to be attributed to the extension of students' rooms. The new building commenced last year for the accommodation of the bookbinders has been completed, and the bookbinding staff has been transferred thither. The basement rooms of the Museum which have thus been vacated are being fitted for the storage of newspapers.

The part of the return relating to the British Museum (Natural History) states that the total number of visitors to the Natural History Museum in 1898 was 419,004, as compared with 422,607 in 1897. The slight falling-off shown in these figures has taken place in the week-day visitors, the Sunday attendance for the year (50,432) being a little in excess of that for 1897. The average daily attendance for all open days during the year was 1,151; for week-days only, 1,181; and for Sundays, when the Museum is open only in the afternoons, 970. The new whale room was opened to the public on Whit Monday, May 30, 1898, and has been much appreciated by visitors. A photographic studio has been erected at the back of the Museum. Renewed application has been made to the Lords Commissioners of the Treasury for funds for lighting the galleries and studies of the museum by electric light, and a sum of £2,000 has been provided for 1899–1900 for the introduction of the light into the work rooms and studies.

Important acquisitions by purchase have been made during the year, among which special mention may be made of the Norman collection of marine invertebrates and land and fresh-water shells (first instalment, consisting of over 26,000 specimens); a complete skeleton of an aboriginal Tasmanian, a race now extinct; a specimen of the rare mollusc *Pleurotomaria beyrichii* from Japan, the only living specimen yet discovered; an entire specimen of the rare elasmobranch fish, *Squatina alifera*, from the lithographic stone of Nusplingen; a valuable and unique collection of fossil insect remains formed by the late Rev. P. B. Brodie (4,700 specimens); the Piper collection of fossils from all the strata of the Ledbury Tunnel (1,806 specimens); and a selection from the late Rev. T. T. Lewis's collection of old red sandstone fishes, &c., of historic interest as having been specially studied and referred to by Sir Roderick Murchison. The number of separate presents reported as having been received during the year by the several departments of the Museum was 1,610, as compared with 1,622 in the preceding year. Many of these comprised a large number of individual specimens.

UNIFORMITY IN SIZE OF PAGES OF SCIENTIFIC PUBLICATIONS.

A committee of the British Association for the Advancement of Science was appointed in 1896 to secure, if possible, uniformity in the pages of scientific transactions and journals. It has already issued one report on the subject, and now, in a circular letter, strongly recommends that there should be but two standard sizes, octavo or quarto form, with the following dimensions, as issued with pages uncut:

1. *Standard Octavo Size.*—Pages 14 by 22 cm., or $5\frac{5}{8}$ in. by $8\frac{3}{4}$ in.

From stitching to outer margin of letterpress, 12 cm., or $4\frac{5}{8}$ in.

Height of letterpress including running headline, 18 cm., or 7 in.

Limits: pages not less than 14 by 21.5 cm., or $5\frac{1}{2}$ in. by $8\frac{1}{2}$ in.

Letterpress not more than 12.5 cm., or $4\frac{7}{8}$ in., from stitching, and 18.5 cm., or $7\frac{1}{4}$ in., high.

2. *Standard Quarto Size.*—Pages 22 by 28.5 cm., or $8\frac{3}{4}$ in. by $11\frac{1}{4}$ in.; letterpress 18.5 cm., or $7\frac{1}{4}$ in., from stitching to outer margin of letterpress, and 21.5 cm., or $81\frac{1}{2}$ in., high.

Limits: pages not less than 21.5 by 28 cm., or $8\frac{1}{2}$ in. by 11 in.

Letterpress not more than 19 cm., or $7\frac{1}{2}$ in., from stitching, and 23 cm., or 9 in, high.

In order to secure satisfactory binding together, the printed area should be small enough not only to escape being cut into, but also to leave a reasonably large margin, and the paper used should be large enough always to reach to the cut edge of a bound volume. Plates should be folded within the standard sizes so as not to be injured when the edges of the book are cut in the binder's press. It is also recommended that every article should always begin at the top of a right-hand page, even if that involves a blank left-hand page, so that a paper can be extracted from a journal without mutilating one or the other.

We fear that these recommendations can scarcely be carried out in the United States. A majority of our leading scientific journals are of a size almost exactly intermediate between the standard octavo and standard quarto forms. The convenience of this size seems to be indicated by the fact that it has been chosen by the committee for the publication of their report

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STANDARD MEASURING INSTRUMENTS.

The Committee of Standards for Instruments of Measure, of the American Chemical Society, consisting of Messrs E. E. Ewell, chairman; Louis A. Fischer, H. P. Talbot, C. E. Linebarger and G. E. Barton, have drawn up a report which has been adopted by the Council. This is as follows:

Your committee, to which you have assigned the duty of making a study of the means by which the American Chemical Society can hasten the adoption of uniform systems of graduation, definite limits of accuracy, and standard methods for using all forms of measuring instruments employed in chemical laboratories, beg to make the following preliminary report:

The committee was promptly organized by correspondence after its members had been notified of their appointment by the proper officer of the Society. After much discussion the com-

mittee decided to take up first the consideration of the proper form, system of graduation, limits of accuracy, manner of labelling, and methods of using glass volumetric apparatus. The committee has made a careful study of the work that has already been done in other countries on the subject, an account of which is given on pp. 527–550 of the Journal of the Society.

Your committee accordingly submits the following recommendations for your consideration:

1. That the American Chemical Society, in a manner consistent with its constitution and by-laws, ask the U. S. Office of Weights and Measures to adopt regulations for the verification of volumetric apparatus which shall be similar in purpose and scope to the regulations of the Kaiserliche Normal-Aichungs-Commission, after due consideration of the criticisms to which the latter have been subjected.

2. That the U. S. Office of Weights and Measures be asked to give special consideration to the question of a standard temperature or temperatures to be adopted for the graduation of volumetric apparatus, and to obtain as far as practicable an expression of opinion from American chemists on this point.

3. That the U. S. Office of Weights and Measures be asked to submit its regulations to the American Chemical Society, or a duly appointed committee thereof, for suggestions before final adoption by that office.

4. That the international kilogram be adopted as the standard of mass.

5. That the liter as defined by the International Committee on Weights and Measures, be adopted; *viz.*, the volume of the *mass* of a kilogram of pure water at the temperature of maximum density and under a pressure of 760 mm. of mercury.

6. That all density determinations be referred to water at its maximum density and under a pressure of 760 mm. of mercury.

7. That all temperatures be expressed in terms of the hydrogen thermometer of the International Bureau of Weights and Measures.

8. That if any question arise as to the interpretation of the above definitions the decision and standards of the U. S. Office of Standard Weights and Measures shall be accepted by the Society as final.

SCIENTIFIC NOTES AND NEWS.

THE American Association for the Advancement of Science is holding, at Columbus, Ohio, its 48th annual meeting as we go to press. We are able to publish, this week, the address given on Monday by the retiring President of the Association, Professor F. W. Putnam, and the address, before the Section of Physics, of the Vice-President, Dr. Elihu Thomson.

PROFESSOR ROBERT WILHELM EBERHARDT BUNSEN, the great chemist, born at Göttingen, on March 13, 1811, died at Heidelberg, on August 16th.

THE death is announced of Sir Edward Frankland, K.C.B., F.R.S., the eminent chemist. Born in 1825, he was educated in the Royal School of Mines, London, and in German universities under Bunsen and Liebig. He was successively professor of chemistry at Owens College, Manchester; at the Royal Institution, London, and at the Royal School of Mines, London. He was the author of works on chemistry and water analysis, and is perhaps best known for his inquiries into the polution of rivers and his reports on the water supply of London. He had been President of the Chemical Society and was Honorary Secretary of the Royal Society. His son is Dr. Percy Faraday Frankland, F.R.S., professor of chemistry at Mason College, Birmingham, and a leading authority on bacteriology.

ON August 2d Queen Victoria conferred the honor of knighthood upon Sir William Henry Preece and Sir Michael Foster, Knight Commanders of the Order of the Bath.

THE Neill Prize for 1895–98 has been awarded to Professor J. Cossar Ewart, M.D., F.R.S., by the Royal Society, Edinburgh, for his experiments and investigations bearing on the theory of heredity.

THE Pharmaceutical Society of Great Britain has awarded the Hanbury Gold Medal to Professor Albert Ladenburg, for his work on alkaloids and their derivatives.

THE Alvarenga Prize of the College of Physicians of Philadelphia has been awarded to Dr. Robert L. Randolph, of Baltimore, for his essay entitled 'The Regeneration of the Crystalline Lens: an Experimental Study.'

Dr. William Z. Ripley has recently been elected a corresponding member of the Società Romana di Antropologiá and of the Société des Sciences Mathematiques et Naturelles de Cherbourg.

Nature states that Mr. J. S. Budgett, of Trinity College, Cambridge, who accompanied Mr. Graham Kerr on his expedition in search of *Lepidosiren*, has been successful in obtaining eggs and larvæ of the Crossopterygian Ganoid *Polypterus*. From a short account of his investigations, illustrated by sketches, which Mr. Budgett has sent to England, it appears that the larva is very minute, and possesses a 'cement organ' on the dorsal surface of the head.

The death is announced of M. N. Rieggenbach, Correspondent of the Paris Academy of Science in the Section of Mechanics.

The will of Mr. George Averoff, who died, at Alexandria, on July 27th, gives large sums to educational and other public institutions. Among these is a bequest of £20,000 to create an agricultural school in Thessaly and one of £50,000 to the Polytechnic Schools at Athens.

A British departmental committee, with Sir Hubert Maxwell as chairman, is investigating preservatives and coloring matter in food.

The Commissioners for the Exhibition of 1851 have made appointments to Science Research Scholarships for the year 1899, on the recommendation of the authorities of the respective universities and colleges. The scholarships are of the value of £150 a year, and are ordinarily tenable for two years (subject to a satisfactory report at the end of the first year) in any university at home or abroad, or in some other institution approved of by the Commissioners. The scholars are to devote themselves exclusively to study and research in some branch of science, the extension of which is important to the industries of the country. A limited number of the scholarships are renewed for a third year where it appears that the renewal is likely to result in work of scientific importance. There were this year six students appointed for a third term, twelve reappointments and sixteen new appointments. Of the two classes first mentioned the place of study is designated. This is as follows: Cambridge University, 6; Owens College, Manchester, 3; Leipzig University, 3; Central Technical College, London; University College, London; Harvard University, Berlin University and the Marine Biological Laboratories, each 1.

The Labrador mail steamer reports that the Peary expedition steamer *Diana* passed Domino Run, northern Labrador, at two o'clock in the afternoon of July 24th. All on board were well at that time. The *Diana* hoped to reach Disco, Greenland, by July 29th.

A reunion of the British Institution of Electrical Engineers is arranged to be held in Switzerland from the 1st to the 9th of September inclusive. The *Times* states that the members will assemble at Basle, and on September 2d will visit the Rheinfelden electrical power station, and will proceed on the same day to Zurich, where they will remain until September 6th. During that time visits will be made to establishments and places of electrical interest, including the works of the Oerlikon Company, those of Messrs. Brown, Boveri & Co., the Dowson gas-generating station of the Zurich-Oerlikon Street Railway, Messrs. Escher, Wyss & Co.'s works, the municipal central electric lighting station and sub-station of Zurich, the new electrical power station at Schaffhausen, and the Falls of the Rhine. There will also be a visit to the iron and steel works of Messrs. George Fischer and to the National Museum at Zurich. In the evening of September 5th a banquet will be given to the visitors by the Swiss firms and the Schweizerischer Elektrotechnisher Verein. On the evening of September 6th the members will leave for Lucerne, where they will inspect the street railways, the Rathhausen Works, and the Stansstad-Engelberg Railway. They will then proceed direct to Interlaken, via the Brünig Pass, and on September 8th will visit the Jungfrau Railway, via Lauterbrunnen and the Wengern Alp Railway, the power station at Lauterbrunnen also being inspected. On September 9th there will be a visit to the Kander Werk at Spiez (central station for light and power distribution) and to the Burgdorf-Thun Electric Railway, which will bring the reunion to a close. In order to con-

tribute to the comfort of the members, especially the ladies of the party, it has been arranged that evening dress will not be required during any portion of the visit.

THE opening meeting of the Sixth International Otological Congress took place in London on August 8th, under the presidency of Professor Urban Pritchard, who delivered an address on the 'Birth and Growth of Otological Science.'

RICHARD ANDRE, in an editorial note in a recent issue of *Globus* deprecates the term 'Amerind' that has been proposed by the Anthropological Society of Washington in place of the current terms 'American Race,' 'Indians' and 'Red Men.' He doubts the necessity of introducing a new term and considers the change as arbitrary and as 'unhistorical.'

AN extensive series of experiments has been in progress during the summer, in the neighborhood of Shelter Island, by Mr. John P. Holland, the inventor of the Holland Torpedo Boat, in the investigation of the various problems of submarine navigation and warfare. The Holland submarine boat has been kept busy in these experiments, and it is said has performed remarkably well. This is the craft lately built at Mr. Nixon's shipyard at Elizabethport.

Nature quotes from the special number of the *Atti*, containing the report of the anniversary meeting of the Reale Accademia dei Lincei, announcing the annual awards of prizes. The Royal prize for astronomy for 1896 remains unawarded. The Royal prize for philology and languages is divided equally between Professor Pio Rajna, for his critical edition of Dante's 'De Vulgari Eloquentia,' and Professor Claudio Giacomino, for his studies on the Basque language. The prize for history and geography is unawarded, and the same is true of a prize offered for 1898 for perfecting the theory of motion of a rigid body. The Ministerial prize of 3,400 lire for history for 1898 is divided, a prize of 1,700 lire being awarded to Professor Gaëtano Salvemini, and smaller awards being made to Professors Alberto Birro, Niccolò Rodolico and Michele Rosi. Of the Ministerial prize of 3,400 lire for mathematics for 1898, a prize of 2,000 lire is awarded to Professor Ettore Bortolotti, and awards of 700 lire each are made to Professors Federico Amodeo and Francesco Palatini. The adjudicators state that the works of Professor Pirondini would have gained an award had not some of them received recognition on a previous occasion. The adjudicators of the Ministerial prize for philosophical and social sciences for 1897 award 500 lire each to Professors Albino Nagy, Luigi Ambrosi and Tarozzi. The Mantellini prize is unawarded. Of the Santoro prize for electro-technics, one-half is awarded to Signor R. Arnò, for his share in the joint invention with the late Professor G. Ferraris of a new transformer. The Santoro prize for chemistry as applied to agriculture is unawarded, and from the Carpi prize for mathematical physics for 1897–8 a sum of 500 lire is awarded to Signor C. Canovetti, for his papers on the direction of aërostats and on the the resistance of the air.

THE London *Times* reports that the city and environs of Rome were visited on July 19th by a prolonged and relatively severe shock of earthquake, which, while damaging various edifices in Rome itself and doing considerable harm at Frascati, Rocca di Papa and other towns on the Alban Hills, fortunately passed without causing loss of human life. The shock occurred between 2:19 and 2:20 p.m., lasting with minor intensity for twenty-five and with major intensity for about six seconds. A dull noise like the sound of a heavy dray being driven rapidly under an archway accompanied the phenomenon, which was mainly undulatory in character. In many parts of the city the terror-stricken inhabitants rushed into the streets and public gardens, where they were soon drenched by a deluge of torrential rain from light gray clouds which gathered almost instantaneously in what had previously been a perfectly clear sky. The city did not regain its normal aspect until 5 p. m. Several minor casualties are reported. A workman was injured by a falling brick; a horse was killed by the collapse of a stable; a fragment of masonry fell from a church, smashing four paving-stones and narrowly missing a passer-by. The Palazzo Sciara and the Palazzo Chigo in the Corso were slightly damaged; a large stone fell

from the Colosseum; the columns in the Forum were seen to rock, but remained intact; a small fissure appeared in the recently-discovered Lapis Niger, but quickly closed again; some unfinished jerry-built houses collapsed in an outlying quarter of the city.

Though the matter is not one of special scientific interest, it may be mentioned that Mr. J. C. Stevens, London, sold recently a good specimen of the egg of the great auk (*Aleca impennis*), which was one of the three formerly in the collection of the Comte Raoul de Berace. This specimen, which is slightly cracked, was figured in the Memoirs of the Société Zoologique de France in 1898, and with additional notes on its history, it also appeared in the *Bulletin* of the Société in 1891. Bidding started at 100 guineas, and at 300 guineas it became the property of Mr. Middlebrook, of Regent's Park. This is the same price which Sir Vauncey Crewe paid for his specimen in 1894. There are in existence about 51 recorded specimens of the great auk's egg.

The India-European Telegraph reports from Allahabad that a case of scientific interest has occurred at Meerut, where a snake-bitten patient was cured by the injection of Calmette's serum, the efficacy of which had already been made probable by laboratory experiments. The patient had all the symptoms of colubrine poisoning fully developed, and the case was so critical that artificial respiration was found necessary until the serum had time to take effect.

Experiments by Professor Tuma and a number of officers of the Vienna garrison to test the possibility of wireless telegraphy between two balloons on July 14th, says the London *Times*, were attended with a certain degree of success. A balloon held captive at a height of 150 metres served in place of the mast used in the Marconi experiments, being connected with the despatching instruments on the ground by a copper wire. The second free balloon carried a receiving instrument and a wire which hung loose 20 metres below the car. In these conditions it was found possible to communicate with the three officers in the free balloon, who signalled with flags that they had received and understood the telegraphic messages. These signals were observed at an estimated height of 1,600 metres and a distance of about 10 kilometres from the despatching station. Owing to the size and weight of the accumulators and the great danger of bringing them into close proximity to a large volume of explosive gas, it is thus far impossible to telegraph from a balloon to the ground or from one balloon to another. On the return to Vienna of the officers a comparison will be made between the detailed particulars noted by them and the report of the actual messages despatched.

An interesting experiment, says the *British Medical Journal*, which was made in Mentone last autumn with the view of diminishing, if not of exterminating, the mosquito—one of the pests of some parts of the Riviera, especially in October and November—is related by Dr. Samways. In an article published in the *British Medical Journal* last September an account was given of a method of using kerosene recommended by Mr. L. O. Howard, Entomologist to the United States Department of Agriculture, for this purpose. The plan depends upon the fact that kerosene, commonly called paraffin in Great Britain, is fatal to at least some of the species which are called mosquitos. A very small quantity dropped on a pool quickly spreads itself over the surface, and, it is alleged, destroys the larvæ, while at the same time it kills any adult female which attempts to alight with the object of depositing her eggs. The efficacy of kerosene has been disputed, as it has been asserted that the immature mosquito is able to thrust the tip of its respiratory apparatus through the thin film of paraffin. The species of mosquito upon which the experiment was made does not appear to have been identified, but it was probably a culex. The larva of culex floats head downwards, while that of anopheles, which is believed to be the bearer of the malarial parasite, floats horizontally, so that there would be, *a priori*, some ground for expecting that the latter would be more easily killed than the former. It was estimated that there were as many as 400 or 500 larvæ in a bucket of water from the tank in Sir Samuel Hayes's villa, where the experiment was made. All were found to be

killed within an hour or two of the addition of five drops of kerosene, and the many thousands in the tank, which was of nearly 300 cubic feet capacity, were killed by a teaspoonful in a few hours. The experiment is so simple and inexpensive that there seems no reason why it should not be tried on a more extensive scale in other places.

ACCORDING to private advices received from India, says the London *Times*, the Board of Trustees constituted last spring to carry out the scheme approved by the Bombay Legislative Council for preventing a recurrence of the plague has already notified for execution four plans of reconstruction affecting a large insanitary section of the city and involving the laying-out of six miles of new streets. The estimated cost of these projects is Rx.1,820,000. They are designed on the recoupment principle, which was essential in view of the magnitude of the building operations which will have to be carried out on both sides of the new thoroughfares. The general idea of the scheme, the initiation of which was due entirely to Lord Sandhurst, who has devoted much time and personal attention to the city of Bombay, is to deal effectually with the insanitary conditions of the place by removing 'rookeries' and 'slums,' and by providing wide thoroughfares in over-crowded localities very much on the lines which have been so successfully followed in Glasgow, Birmingham and other British towns. The Bombay scheme, however, goes further and deals also with a more serious condition of affairs. It includes provision for the extension of the city by reclaiming large areas on the foreshore of the island as well as the opening out of wide roads, the removal of insanitary dwellings, and the erection on a very extensive scale of new dwellings for the working classes. The Board of Trustees, to which is entrusted the task of carrying out this work, is subsidized by the Corporation of Bombay and endowed with the usufruct of certain valuable building areas belonging to government and the corporation, within the city limits; also with the right of reclamation on the foreshore outside the limits of the port. The Board consists of 14 Trustees, partly *ex officio*, partly elected and partly nominated, and all the chief interests concerned are fully represented. It has special powers to acquire property required for or in connection with the several schemes to be undertaken, and power, with the sanction of the government, to raise loans. The scheme involves a new departure of some moment, and its operation will be watched with considerable interest. In a Western city the project would undoubtedly have been left to the ordinary municipal organization, but in India, although local self-government has developed amazingly, it is no disparagement of that principle to question its efficiency for carrying out improvements dealing with vested interests on an enormous scale, and requiring years of persistent effort on systematic lines for their due accomplishment, in addition to the task of administering the ordinary affairs of a great city.

SIR ROBERT S. BALL, in his annual report of proceedings in the Cambridge Observatory for the year ended May 25, 1899, states, according to the report in the London *Times*, that during this period the meridian circle has been used, as in the previous year, for the perfection of the catalogue by re-observing stars, of which not more than two observations had been obtained, in order to carry out the original design that each place should depend on not less than three observations. To this end 2,241 observations have been taken of 1,429 stars; 58 are still insufficiently observed, and five have not yet been re-observed. The intervals of the transit wires were determined afresh, at the beginning of this year, from 115 observations of Polaris made in 1898, and tables were constructed for facilitating the reductions to center wire. These intervals have been used since the beginning of 1899. A very important addition to the instrumental equipment of the Observatory has been made during the past year by the erection of the new equatorial, which will be known as the Sheepshanks equatorial. A machine for measuring the photographs has been designed, and is now being constructed by the Cambridge Scientific Instrument Company. It is essentially a form of the instrument designed by Professor Turner for the work of the astrographic chart, modified to give the greater accuracy required in stellar parallax work. The syndicate have pleasure in announcing that they have re-

ceived from a donor, who wishes to remain anonymous, a donation of £50 towards the expenses of making a catalogue of books in the library. Preparations have been made for the complete arrangement and cataloguing of the books during the present summer. The number of members of the University attending Mr. Hinks's classes in practical astronomy shows an encouraging increase. The Newall telescope has been used for observations on 96 nights in the course of the year 1898 (May 19th)–1899 (May 19th). In November and December there were 33 consecutive nights on which clouds rendered it useless to attempt observations. In continuation of the work referred to in last year's report the instrument has been employed in connection with the Bruce spectroscope in taking photographs of stellar spectra for the determination of velocity in the line of sight. In the course of the year 150 photographs (in addition to many others rejected for various reasons) have been obtained, giving material available for the determination of the velocity of 60 stars. Preparations are well advanced for converting the Bruce spectroscope into a powerful four-prism instrument, which is to be used in securing material for a detailed examination of the spectra of a few of the brightest stars.

UNIVERSITY AND EDUCATIONAL NEWS.

Dr. Arthur T. Hadley will be formally inaugurated President of Yale University on October 18th. The occasion will be of special interest, as Dr. Hadley will deliver an address, which will doubtless outline the policy of the University for many years to come.

The University of Berlin celebrated on August 3d the 90th anniversary of its foundation by Frederick William III. The oration was delivered by the retiring rector, Dr. Waldeyer, professor of anatomy, who took as his subject, 'Does the University of Berlin fulfill the mission entrusted to it by its founder?' Dr. Waldeyer is succeeded as rector by Professor Fuchs, the distinguished mathematician.

Nature states that the Research Fellowships founded by the Salters' Company and the Leathersellers' Company for the encouragement of higher research in chemistry in its relation to manufactures tenable at the City and Guilds Central Technical College, being now vacant, the Executive Committee of the City and Guilds of London Institute will, before the commencement of next session, consider applications and elect candidates. The grant made by each of the companies to the Institute for this purpose is 150*l.* a year. Copies of the schemes under which the Fellowships will be awarded may be had on application to the Honorary Secretary of the Institute, Gresham College, Hasinghall Street, London, E. C.

Mr. Mark W. Harrington will return to his professional work, and would accept a call to a chair of astronomy or mathematics. He could not fail to build up a good department in any of the lines coming under these heads—as in polytechnic schools, or in universities, or in colleges, where new developments in the direction of an observatory or a branch of engineering are contemplated.

The following new appointments and promotions have been made in the French universities: M. Haller, professor at Nancy, to be professor of organic chemistry at Paris; M. Pellat, to be professor of physics at Paris; M. Chatin, to be professor of histology at Paris; M. Cannien to be professor of anatomy at Bordeaux; M. Künstler to be professor of comparative anatomy and embryology at Bordeaux; M. Picart to be professor of astronomy at Lille; M. Ardaillon to be professor of geography at Lille; M. Guitel to be adjunct professor of zoology at Rennes.

Professor v. Frey has been called to the professorship of physiology at Würzberg.

Dr. Hans Battermann, Observer in the Observatory at Berlin, has been promoted to a professorship.

The following have qualified for docents in the German universities: Dr. Somner for physiology in Würzburg, Dr. Schwarzschild for astronomy in Munich, and Dr. Stolle for chemistry in Heidelberg.

Dr. A. C. Houston has been appointed lecturer in bacteriology at Bedford College, London.

Edward Orton

President of the American Association for the Advancement of Science.